Science Policy Study—Hearings Volume 17
RESEARCH PROJECT SELECTION

HEARINGS

BEFORE THE

TASK FORCE ON SCIENCE POLICY

OF THE

COMMITTEE ON SCIENCE AND TECHNOLOGY HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

SECOND SESSION

APRIL 8, 9, 10, 1986

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RESEARCH PROJECT SELECTION

TUESDAY, APRIL 8, 1986

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to call, at 9:08 a.m., in room 2318, Rayburn House Office Building, Hon. Ron Packard presiding.

Mr. Packard. Good morning, ladies and gentlemen. We will begin the hearing this morning recognizing that the chairman is not here yet; he will be coming shortly. Second, Mr. Lujan, who was originally scheduled to chair the hearing, has been indisposed for health reasons and will perhaps not be with us for a few weeks, so I have been asked to chair the hearing.

This morning, the Science Policy Task Force begins 3 days of hearings on "Research Project Selection." As a Nation, we are experiencing a period of decreasing Federal resources available for support of scientific research. During this period of severe budget constraint it is extremely important that we allocate the available funds in such a fashion as to meet our national goals and those of the research community.

Underlying much of the present grant system is the belief that the best scientific results are obtained through competition based principally on potential scientific merit. Since the 1950's, Federal agencies have increasingly relied upon some form of peer review to select research projects for funding.

There are indications that this process may not be particularly effective during periods of slow growth, either nationally or within a specific scientific discipline. Some scientists feel strongly that this system is not well suited for use in all areas of science. There have also been allegations that this highly competitive system by its very nature discriminates against interdisciplinary and multidisciplinary research and discourages high risk and innovative research.

We continue to hear complaints indicating that the administrative workload associated with peer review has increased to a level where the validity of the system may be stressed.

During hearings this week the task force will examine various processes which may be used to select research projects for funding and the advantages and disadvantages of each to determine whether the project selection processes currently in use adequately meet our needs. We will also hear on the Federal granting agencies—on how the Federal granting agencies, private organizations, and industry select basic research projects for funding.

This morning we welcome Dr. Rustum Roy, director of the science, technology and society program at Penn State University. Later on we will be pleased to hear from Dr. Ruth Kirschstein, director of the National Institute of General Medicine Science.

Dr. Roy, we're very grateful that you've come to testify to the committee. For your information, all of the prepared testimony which you have brought to us will be entered into the record, and any other extraneous material you would like to have entered in the record we'll be happy to do so. You may wish to summarize; you may wish to read your testimony. Whichever procedure you'd prefer, we're delighted that you're here and we will now turn the time to you for your testimony.

[A biographical sketch of Dr. Roy follows:]

DR. RUSTUM ROY

Rustum Roy is Evan Pugh Professor of the Solid State and Professor of Geochemistry at The Pennsylvania State University. He also serves as Director of the Science, Technology and Society Programs of the University.

Professor Roy is a crystal chemist specializing in the design, synthesis and preparation of new materials. He is the author of some 400 papers and several books. His work has been recognized by various awards and by his election to the National Academy of Engineering, and as a foreign member of the Royal Swedish Academy of Engineering Sciences and the Indian National Science Academy. Professor Roy has been deeply involved in professional and educational innovation. He has been a major figure in the creation of the national interdisciplinary materials research community, by organizing national meetings and analyses of the field, and by being the principal architect of the Materials Resarch Society and the Materials Education Council. At the University, besides being the founding Director of the University's interdisciplinary Materials Research Laboratory for a quarter century, he played a major role in establishing geochemistry, and later solid state science as academic disciplines on the campus. Most recently he has played the same role in creating and directing the Science, Technology and Society Programs which covers the widest spectrum of disciplines. His innovations in the creation and delivery of new educational teaching materials have international significance for both the materials science and STS fields. Besides his research, his current efforts are focused increasingly on the technological literacy of the general population via the STS route.

Professor Roy has been involved in science policy matters for two decades. He served three terms as a member of the Pennsylvania Governor's Science Advisory Committee. At the Federal level he has served on many panels, Boards and Committee of the NAS-NRC-NAE and the National Science Foundation. He has been a consultant to some industries continuously for over twenty years. He has studied and written widely on matters of ethics and values. He was first chair of the National Council of Churches' Committee on Science, Technology and the Church, and in 1979 he was selected to give the prestigious Hibbert Lectures (in its Centenary year) in London, England, on the relationship of science and technology to contemporary religious insight.

STATEMENT OF DR. RUSTUM ROY, DIRECTOR, SCIENCE, TECHNOLOGY AND SOCIETY PROGRAM; EVAN PUGH PROFESSOR OF THE SOLID STATE AND PROFESSOR OF GEOCHEMISTRY, PENNSYLVANIA STATE UNIVERSITY, UNIVERSITY PARK, PA

Dr. ROY. Mr. Chairman, I'm grateful and honored to be given this opportunity to exercise my citizenship by sharing with you my impressions on these issues. You said two very important words in the statement you just made. One was the decreasing rate of Federal investment in science, and the other was investment in science. I will try and key my remarks to those two words especially. I have no intention of reading this stuff, but I will lead you through it so people will have a feel of the context in which I think the project selection basis is carried on and, of course, I will present an alternative at the end.

You see before you, Mr. Chairman, a two-eyed witness. Normally, we say we get these two-handed witnesses. The guy says on the one hand something; on the other hand something else. Well, I'm a two-eyed witness.

I come with one eye of a "working stiff" scientist. I mean, I'm in the trenches. I look at your list—most of these guys aren't in the trenches. I'm in the trenches. I have about $800,000 of your money and mine, taxpayers' money, every year for the last 10 years. I write about 20 proposals. I get funded. I'm not an unsuccessful peer review scientist. But I still think the system is in very deep trouble. So I'm a successful peer review manipulator and I think the system is in deep trouble.

The second eye, however, is that of the policy analyst. I teach science policy. I've been writing books in the field for some years. And I stand back and ask: "If this is the only way to do it, what do other nations do? Is this efficient? Is this a human way? Is this a politically sound way?"

So I have a bifocal vision, a binocular vision that I will be presenting. I'm a working scientist that also thinks about policy. You have a lot of very distinguished scientists on your roster, but I'm not sure they're in the trenches. Let me give you a little bit of information.

This week, American science is shut down. No; I'm not exaggerating. Everybody is out there writing proposals for the URI's, for the Federal things. We will turn out 27 proposals from Penn State for about $3 million each. Nothing is going on. Let me tell you, there is no science going on. Everybody is writing proposals. The whole physical science and engineering world of the United States is shut down. What are we doing? We're writing proposals.

Well, let me lead you through this. By trying to set a context for what I think is happening, I believe that we have to start by asking: "What is the state of U.S. science and engineering?" The perception of my colleagues who don't have the second view, who are very distinguished scientists, is that, "My God, we're in good shape. We're winning Nobel Prizes like they're going out of style." But the Nobel Prize is no more an index of the health of American science and technology than the Dow Jones is an index of the health of the American economy. We have just as bad deficits in science as you have in the Federal deficit. The carrying charges on our deficits will also go into the future. So it is a question of saying what is the health of U.S. science and technology, not only of U.S. Nobel Prize winners.

Well, I feel, Mr. Chairman, that in a sense the Congress' main job is not only to get to the nuts and bolts of how to select a project, whether you should use the peer review or not, but if you're making an investment, you look at the investment climate. What is the climate? And I try to phrase that in my little testimony here as a series of questions as a wise investment counselor. I'm Smith, Barney. I earn my money. Now, in a sense, is your investment counselor telling you what's going to happen to oil prices? It's no point in just saying what to select. What is the context in which the project selection for the next few years will be carried out?

I start by putting to you these questions.

What is the committee's view of how science connects to technology? Out there there are roughly two views. Of course, they shade into each other, but the first view is that of the science community, which is more or less gospel truth. We know what goes on. We have science that leads to applied science and leads to technology. And I give you a quotation from Mr. Keyworth, which, I submit, is representative of the science community. He was convinced, as are the members of the President's Commission on Industrial Competitiveness, "that science leads inevitably to innovative technology, and thus to prosperity."

Now, that view is irreconcilable, I submit, irreconcilable with the view of historians, of economists, of many of the industrial leaders of the United States that there is any connection between science being produced by a nation and technology which utilizes that science.

Japan is a science-eating machine. We are the science-producing machines. If you want productivity and you want an increase of technology, you don't plant more trees to make more science. You plant more trees that make technology that use science. So the science-eating tree and the science-producing tree are different machines. Mr. Keyworth has them confused. It is fundamentally the view of my own tribe. Most of us as scientists believe that, but it is wrong. Now, you may believe he is right, but the Congress cannot make policy without making a decision about that issue.

There is a second issue. The second issue is an issue of definitude of science. Is it an infinite box out there that will keep on chewing up this thing forever and ever and we'll get goodies out of the storehouse of nature? Or is it in fact finite? If you were in the mining business or in the oil business, you would very much want to know whether you've got a finite pool or whether you've got an infinite pool. It is the assertion, and I give you quotations from people like Victor Weisskopf. I mean, these are not amateurs—Gunther Stent, Eugene Wigner—telling you that usable science is finite. Remember that investment good you alluded to. If you want to make an investment, then the question very much is: "Is it a finite pool or an infinite pool?" Because your investment strategies would be radically different. I submit, again, that a kind of gospel view is that science is an infinite pool. I also submit that many people think that is wrong. That the kind of science which human beings can utilize, that science—I use my words carefully, as Weisskopf has done—there is a mistake in his quotation, by the way—that it is indeed finite.

Now the third question is what is the trendline in costs? Is the cost per unit science going up, or down? Well, it is going asymptotically. I mean, it's just going out of sight—exponentially out of sight—the cost per unit useful discovery by any size. We've got all kinds of numbers which say it's getting more expensive to get out little bits of science which we can put to work. So one has to know that question, which way one does believe that.

The fourth question I put on here is, we have had a lot of my colleagues allege that what American science needs is instrumentation. We need more gizmos, gadgets, and so on. Now I am in the materials business. I directed the biggest materials lab in the country for a quarter of a century and I think I know all that. It's a hot field of science today. I don't think any of us really is hurting for instruments. You know what we are hurting for? It is time to think. There is no time to think. We're running from one thing to another. Now you're saying, what's this? This guy comes in here and wants time to think. I believe and I suggest very seriously, I've said it to Mr. Bloch, we should have grants for faculty members to take half a day off, shut the door and think. The most efficient process that the human being can do is to think. It costs nothing. But we have no time to think.

Now, you say, "Well, would I rather have instruments than think?" Well, we have a little cabal, many of us scientists from Stanford and Cornell. We've got a little team saying, "Let's stop this nonsense toward more instruments." Then I believe we should ask the question, "Are we investing in the right places?" And I suggest that we need a little bit of thought before we get into that.

The way we can use instruments, and I would suggest that obviously I'm not against using instruments—we are very well equipped—is to share it. I do most of my magnetic angle scanning work in Cambridge, England. It is cheaper by a factor of 10 than getting a new machine for us. I do all my supertransmission microscopy at Arizona State. My friend John Cowley is the world's leader. I just send my graduate students, and ship them out there and they use that.

We haven't learned to cooperate. And these big supercolliders—nothing should be built like that without international cooperation. I'm not suggesting that science should shut down machines. I'm suggesting that, like automobile industry learned to down-size, the science community, in an era of decreasing budgets in your words, Mr. Chairman, has got to learn to down-size the operation, to increase its efficiency and not just ask for more money. I suggest that sharing of equipment is one of the ways we can do it. We all know how to do it. If the people don't know how to do it, they are not in the business.

Now the last question with respect to investment is whom are you investing for, the Nation or the individual? If you're saying the individual is to be protected and his reputation, then I think our process is OK. But does the Nation gain by giving money to scientists who will say my service is only to my profession and to myself? Why is public funds expended? I'm not a Milton Friedman supporter, but Mr. Friedman, at the beginning of this administration, asked that question: "What is the investment good for in basic science?" I think that question has never been answered and it needs some kind of answer. Is it only a consumption good? Or is it an investment good? If you're talking about investment, one has to look at that.

I come to question 6 which says, "Is the broad distribution of the fields unchangeable?" Did God give us this pie and say, "Oh, my golly, you got so much for particle physics and so much, and nobody can change it?" But I've been trying to change that engineering component of the NSF for the last 18 or 20 years. It's never worked. So what is the ratio that we should have, and can we do it given the present institutional framework? One has to answer that question.

My last question is question 7 which is on page 51, of your scoping document for which I commend John Holmfeld and all these other staff people because it is an extremely wise document. For the first time I see the right questions being raised. And I see in that it says, "Do different fields of science require different systems?" The answer is yes. Engineering can't be judged by science criteria, but it is being judged by science criteria.

Let me turn to what I think we might be able to do with these. What do we know about project selection? I suggest that the biggest, greatest invention of the U.S. science policy effort was multiple sources, not peer review but multiple resources. I also suggest that it is good because of the role of chance. Now scientists don't like to hear about that, but the role of chance in discoveries—we'll have a whole session of the AAAS on this—is important. Because you know if you throw the dice more times with more agencies you have a better chance. So it's not trivial to say that more agencies is very important.

Of course your documents point out that there are so many different systems in use, and you'll have people from ONR here using the strong manager system. It has nothing to do with peer review. And I submit that one of the peculiar things is why nobody has ever compared for the consumers: Which system do you like better, a peer reviewed or a strong manager? It's a simple thing. The Congress could ask for it. To have a $50,000 study made of do you get better work, do you get more citations, more papers? Do you have more distinguished scientists used by the ONR system or by the NSF system? That doesn't take much. But you've never had that. That is the most salient index of the efficacy of peer review, I suggest, than are all the studies of whether or not we gave the right grants to the right people.

We have learned a lot in the last 25 years about what our systems do, and I suggest that we really should look critically at that, for instance, in block funding, as we get into that. In this week's National Academy of Engineering thing there is a review of the block funding of the materials labs. This is 25 years' old. They didn't do very well on interdisciplinarity. They didn't do very well on coupling to industry. But they did do very well on being efficient and saving the taxpayers' money. So we shouldn't expect them to do some things, but we should expect them to do others.

Well, I've critiqued the peer review system for 20 years and you've got two papers from me. I don't intend to repeat those. I do want to say it has nothing to do with the usual criticisms. I also want to say I'm not in the biomedical sciences, but in physical science and engineering I think the case is inefficient. It is wasting the most precious resource of the United States, which is the time of the best scientists in the country. That's what's wrong with it.

I turn to the alternatives. I believe this is on my page 14. I think that Congress must take a much broader look at the relative dollar allocations between, say, science and engineering, or science and applied science. I believe it is too far tilted toward the consumption good and not the investment good. I think it should take a look at what is basic science, and look at the question of how defense science is giving us spinoffs, which is one of those myths which has come down to us from the sixties. Does it, in fact? When we teach policy we show the negative correlation between investment in defense spending and in productivity growth. That is so well established it's the kind of first thing that the student learns. And the Congress has to look at that.

I also think that the Congress should mandate new selection procedures as experiments. We don't know what is good, Mr. Chairman. We haven't the foggiest idea what's the best. But we do know if we don't try we're never going to find something better. And so far we've seen resistance to trying alternative methods. And your hearing documents suggest that it's time to look at something else.

Well, what is an alternative? I believe that the strong manager system used by the Defense Department, for instance, is a viable working alternative. Industry doesn't use peer review. It seems to get along very well. Quantum mechanics was not discovered by peer review, nor relativity. Nobody was looking over Einstein's shoulder and sending out a bunch of papers to be looked at. So it's nonsense to allege that peer review gives us good science. There are many ways to get good science.

The formula that I suggest tries to capture the idea of peer review and make it efficient. It was Congressman Miller, Chair of this committee 20 years ago, who first brought in the formula idea. He realized what we are talking about today: how inefficient the system is. He brought in, however, an input formula. My formula is unique in that it is the first formula—I've presented this 50 times to major audiences of 1,000 and 2,000 people and I have yet to get the first criticism, but no action.

Is it, in fact—is there something wrong with it? I said please tell me what you find wrong with it. I give you three things. First and most important, it gives the Congress a real way to tune the system. At present we have captured you. The science community manipulates the Congress: "We say this is good for science, you guys don't know what's good for science, send money, don't ask questions." Now we're saying no; the Congress must have some degree of tunability. I offer you in those coefficients, which is—say, you want to say we want more interaction with industry; you turn up that coefficient a little bit. You want to say we want less biophysicists and more agricultural scientists; you turn up that coefficient a little bit. That's a very easy policy instrument.

I say it is peer reviewed because the productivity of a scientist is peer reviewed all the way through. His Ph.D.'s are peer reviewed by a committee. That is one of the things you want from us, trained personnel. My papers are peer reviewed by the Physical Review editors. So I'm giving you a peer reviewed formula system which gives us the objectives of efficiency, of getting a consensus by the community, it has no ups and downs. Science shouldn't go up and down, it should gently change. The time constant of science is 15 years and we fund things for 1 year. So it will get a much better distribution. Every small college can get a little bit of money. That stimulates science. Most importantly, it is pro innovation. Nobody tells me what to do with that money. I don't have to listen to some character out there who's thinking this is the fashion this week. Every scientist can therefore do his own thing if he produces. The difference between my formula and everyone else, it's productivity linked.

So I think there are many ways in which we can do it. I don't think that formula is any panacea, but I do believe that we should try some new systems.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Roy follows:]

Testimony of Professor Rustum Roy
The Pennsylvania State University
before
The House Science and Technology Committee
April 8, 1986

QUALIFICATIONS OF WITNESS

Dr. Rustum Roy, one of the country's leading materials scientists, is Evan Pugh Professor of the Solid State, Professor of Geochemistry, founding Director of the Materials Research Laboratory (the nation's largest) for 25 years, and Director of the Science, Technology and Society Program--all at The Pennsylvania State University.

He is simultaneously a currently active scientist with some 400 papers, dozen patents and several books to his credit, a senior research administrator intimately involved with U.S. industry, and a science policy analyst. In the latter capacity he has for 25 years served on major National Research Council policy committees, served four Pennsylvania Governors as a senior member of the Governor's Science Advisory Committee and was a Science Policy Fellow at the Brookings Institution in 1983. He has written widely in the science policy field especially on the subjects of interdisciplinarity, university-industry interaction and alternatives to proposal peer review and the absence of a U.S. technology policy.

RESEARCH PROJECT SELECTION

Mr. Chairman, your staff has instructed me that the Committee in these three days will take a look at different ways of Research Project Selection and that my task is to put the issue in broad perspective. My biases are enumerated in the witness qualification statement and my previous writings on this topic found in the references.

I start with a few counter-intuitive observations. That the "Science" establishment in the U.S. is judged to be successful using certain widely accepted criteria (such as securing Nobel prizes) is incontestable. But I also note that the same was said of the U.S. automobile industry right through the height of its gas-guzzler phase into the early seventies.

I am one of the applied science and engineering community that does not believe that taken as a whole, U.S. "Science, Applied Science, Engineering, Technology"--all separate societal functions concealed under the word "Science" is at all healthy. Nobel prizes and citation studies are no better an index of the health of U.S. "Science" than the Dow Jones Index is an accurate index of the health of U.S. economy. We have deficits in "science" as large as the Federal deficit and an equally large negative trade balance. We have lost--I use the past tense with full deliberation--the race in industrial technology just as surely as we will have gone from the greatest creditor to the greatest debtor nation this year. We have an enormous shortfall in the mix of properly trained personnel to sustain our education and our industry. The "deficits" in our precollege science education for the masses, and our postgraduate education in engineering are comparable to the foreign trade deficit--and there is absolutely no cure in sight for a decade or two. Some call such observations the voice of a Cassandra but they forget that Cassandra, who correctly predicted the results of the Trojan horse ploy, had two "hexes" on her--first no one would believe her and, second, what she predicted always came true.

We Americans are suckers for good news. The good news I have to offer the science and technology community is of the kind that Winston Churchill offered Britain: Blood, sweat and tears. First, the science research leadership community must own up, of course with the wisdom of hindsight, to earlier errors of judgment; second we must bite the bullet of increasing efficiency within our system (just as we downsized our automobiles) and not always asking for more money, by making major and not cosmetic changes in how we select projects including the redistributions among fields within the fixed pie; and, third, we must dig in for sweating out lean budgets for two or three decades.

I turn now to the context within which "project selection" is being carried out. I believe that the latter cannot be discussed outside the former. I find that unless one answers the following questions regarding the context of the research to be done how the project is selected is secondary.

Q1. What is the policy maker's view of the relation of Science to Technology?

I begin by asserting that Vannevar-Bush's report to President Roosevelt, "Science the Endless Frontier" has been grossly misinterpreted to provide a wrong theory of how science relates to technology. This theory has it that there is one enterprise called science and technology and that science leads to applied science which in turn leads to technology. The vast majority of American citizens and probably Congresspersons believe this widely publicized myth. The vast majority of American scientists "know" it to be true and proclaim it as gospel. I give you only one quote from the immediately past Science Advisor to the President to show how firmly such error is entrenched at the highest levels.

> "He was convinced, as are the members of the President's Commission on Industrial competitiveness, said Keyworth, that science leads inevitably to innovative technology, and thus to prosperity."
>
> Physics Today, Apr. '85

The sad, and terribly dangerous, part is that if our policies are made on the basis of such a grossly erroneous theory, it won't matter how we select projects to move us in the wrong direction.

I show in some detail in my book "Lost at the Frontier" co-authored with Deborah Shapley that this idea that science leads "inevitably" to technology is simply wrong. The great U.S. historians of science from Yale to Georgia Tech have been saying so for years. In the last few years the engineering community has been slowly asserting itself and has pointed out that there are two separate human communities: of science and of engineering. Their goals, their research methods, their values, their rewards are different. Lastly, the Japanese have conclusively proved in our own marketplace that science is neither necessary nor sufficient for technological innovation and advance. Within the last week in the National Academy's "Issues in S&T" Journal I found this quotation agreeing verbatim with my thesis.

> "One U.S. response to the specter of foreign competition has been a renewed emphasis on university-

> based science and technology training and research. Unhappily, strategic sector analysis suggests that unless the creators of new science and technology are also best at applying them, much of the benefit will be as helpful to Japanese firms as it is to U.S. firms. Indeed, if the contemporary Japanese experience suggests anything, it is that there is no particular connection between the creation and application of technology."
>
> WHY JAPAN IS WINNING, Gary R. Saxonhouse

Q2. Is the storehouse of scientific knowledge finite or infinite?

Almost every policy planner assumes, without much analysis or thought, that there is an infinity of basic knowledge out there accessible only by the route of scientific inquiry and if we just keep looking, "something" more will show up. But there is a small number of distinguished scientists--especially those who have thought and written about it--who do believe that scientific knowledge is finite, more like a major ore body or an oil basin. We can--and are--mining it out, rapidly. Certainly all the "richest" deposits are being attacked by a huge and growing international army of scientists and the best ore will soon be gone. Of course, there will always be leaner and leaner ore and the scrap heaps to be reworked. Here is how Victor Weisskopf, once Head of Physics at M.I.T., and Director General of CERN, puts it:

> "Very few scientists today would maintain that there are new fundamental principles to be discovered with regard to life or the other phenomena I have mentioned.... Nonetheless there is today a general disbelief that the basic principles of the atomic world are known and no additional law or principle is necessary in order to explain the phenomena of the atomic realm, including the existence and development of life.

And even though most scientists expect the new 'revolutions' to come in the field of biology, it is Gunther Stent, the molecular geneticist at the University of California, who writes in his *Paradoxes of Progress*: 'Scientific knowledge, like our Universe, must be finite and the most significant laws of nature will soon have been discovered' and concludes that progress as we have known it is nearing an end.

And here is Eugene Wigner, Nobel Laureate in Physics in an essay on "The Limits of Science":

> "Thirst for knowledge, curiosity concerning the extent of one's mental faculties, and a healthy sense of rivalry, are strong stimulants of the young scientist and will continue to spur him along alos in the future. They are, however, not his only motives: the desire to improve the lot of mankind, to extend its power, is also a traditional trait of scientists. These latter incentives are, however, waning, at least as far as the natural sciences are concerned, with the advent of man's full mastery of the elements, with the increasing realization that the *economic welfare of man is a question of organization rather than a problem of production*." (Emphasis added).

Q3. What does the trendline in the cost per unit scientific discovery (however defined) look like? Will the public keep paying for science whatever its cost and its relevance to their daily life?

Here most would agree that it is getting more and more expensive to do "modern" scientific research, and we are approaching a regime where the next little bit of science could cost billions of dollars (e.g. the supercollider).

As science relevant to societal wishes becomes rarer and the esoteric or baroque (from the public's viewpoint) sciences get more expensive, selling it to the public will become increasingly difficult. Continuing to do so will
discredit all of engineering and science.

Q4. Which is in shorter supply (relatively) in American science--thinking and idea generation, or instrumentation and machines?

It is conventional wisdom that our Universities are in serious need of equipment and facilities. Since there is no agreed-upon definition of need I do not contest this. But I do know all the leading University labs in materials research--one of the hottest research areas today--I cannot think of one which is stopped from doing cutting-edge research in most fields because of lack of equipment. There is a much, much more serious lack in these same departments. That is time for the faculty to think and reflect or plan, really innovative experiments. Faculty are run ragged with proposals, selling themselves to industry or government, serving on committees, worrying about keeping their students fed. I argue that it would both advance science faster (and be cheaper) to substitute in the leading University groups for "equipment grants"--"thinking-time grants." The NSF could have a program to buy one-half day a week of guaranteed isolation and thought for our active scientists: no overhead, no equipment, nothing else. I have yet to have any active scientist disagree: yet no agency or planner even considers such real inexpensive innovation seriously.

Q5. Does funding scientific research help the nation or mainly the individual and the field?

Science is thoroughly international, and its results are distributed at the speed of light all over the world. Moreover, it is the technology machine that utilizes new science best that gets the benefit of any new science wherever produced. A national S&T policy with more science-producers than science-using-technology units is obviously absurd from the nation's viewpoint. But that describes the U.S. system perfectly. Of course, the individual scientists still get the benefit, of more fame or money for science as does the particular scientific field. It is therefore highly unlikely that individuals and fields that get goodly shares of the funds will point out how little value their craft, and even fame, return to the nation's technological sector.

Q6. Can the broad distribution among fields (particle physics, chemistry, electrical engineering, biology) be changed slowly but substantially i.e. >50%, or not?

By the quirk of history that the atomic bomb had a highly visible part in the very end of the war, the physics community became dominant in science policy and thence science funding in the U.S. It is interesting to speculate on the entire course of science had a chemical or biological invention helped end the war. But the project selection issue is played out against a background that no one except Congress can change the division of funds among disciplines more than a few percent. The engineering and scientific community is frozen in place. It is my opinion for instance that in broad outline a healthy U.S. R&D system would require in the University sector something like 40% for science, 40% for engineering and 20% for science/engineering education. Nationally the percentage for engineering should be even higher. Yet one hears serious proposals to fund the SSC for $6 or $7 billion--a sum which could save a major part of the steel and other materials industries and retain hundreds of thousands of jobs. Congress is the only level of decision making at which this issue can be debated.

Field selection by Congress must come before project selection by agencies.

Q7. Do different fields of sciences require different systems?

This question is raised on p. 51 of the Committee's scoping document. It is extremely relevant and important. The answer of course is yes, and that is NOT enforced today. By and large there is a bias towards "science" and against applied science and "engineering"; there is a bias towards abstraction and analysis and against inductive experimentation and synthesis.

Again only the Congress can change this by legislation of simple guidelines. Without it the changes have not happened over a 20-year period even though advocated by many senior powerful individuals and will not happen. (I make some proposals later.) Prominent individuals like Hendrik Bode, Edward Teller, Arthur Kantrowitz remarked on this bias against applied science in a report to this Committee in 1965 (Basic Research Goals, 1965).

WHAT DO WE KNOW ABOUT THE PROJECT SELECTION SYSTEMS IN USE IN THE U.S.?

1. By far the most important invention in engineering science funding in the U.S. was the development of multiple sources.

2. Theoretically this is very important because of the much-neglected but statistically most important role of CHANCE in fundamental scientific research. More agencies means more throws of the dice are possible.

3. There are many systems in use in the country. The vast majority of the decisions on what research to fund are made by "strong managers"--in industry, in DoD and other mission agencies. There is considerable formula funding, as in agriculture. And there is a set of procedures loosely grouped under the term peer-review. A lottery among qualified individuals or groups--the Biblical system--has not seriously been tried, but is definitely deserving of study and experimentation.

4. The total system entrepreneurship, activity and salespersonship on the part of the scientist engineer, not all of whom are equally endowed in this area.

5. It is a remarkable omission but not a single study has ever been made which compares the "consumer's" satisfaction with the different Federal systems. Nor has any study been made of the quality or productivity of researchers funded by, say, the ONR using the strong manager approach and the NSF using mail peer review in the same field. A comprison of the efficacy of at least two such systems could be available to the Committee in six months.

6. Likewise there is a substantial track record with "block funding" which the committee should examine directly. The nation's

Materials Research Laboratories are now 25 years old. There are about 30 of them, roughly half block funded and half not block funded originally by ARPA, later taken over by NSF. The National Academy of Sciences COSMAT study evaluated these organizations in 1972-4. In 1979 NSF funded the Mitre Corporation to do a similar study (see Refs.). What can we learn from these studies in general?

(a) Block funding saves a great deal of faculty and agency time with respect to total "management," from proposal to monitoring.

(b) The quality of research is neither better nor worse than in similar projects funded individually.

(c) Two major goals--interdisciplinarity and coupling to industry--originally envisaged by ARPA (and now very prominent in NSF ERC thinking) were not attained to a satisfactory degree. (Interdisciplinarity was even dropped from the title when the labs were transferred to NSF.) The non-block funded Universities did much better on coupling to industry.

7. We have noted above the bias in the selection system against "applied science," called to his committee's attention in 1965. This bias persists and costs the nation very, very dearly. Because it is a key missing link in the chain which will enable U.S. industry to better utilize the fundamental new knowledge we generate in our basic science efforts.

8. The selection system reflects most Universities' inability to handle interdisciplinary research effectively. In spite of the fact that virtually all societally significant research demands

interdisciplinary interaction in general the University's track record so far is disastrous (Roy, 1977). Agencies have been unable to "entice" or "bribe" Universities to change their internal discipline-dominated values to make interdisciplinarity really work on a campus.

A small percentage of Universities have indeed learned to do this well. It is not clear that all should. But Federal money for projects demanding interdisciplinarity must surely be carefully restricted to those Universities that have demonstrated their ability to genuinely foster such a mode. Group, applied science and engineering, can effectively be done today only at a small set of Universities. Pretending that the other leopards will change their spots is bad national planning. Against as Dr. P.H. Abelson, long-time Editor of Science put it "Performance (i.e. previous track record) not Promise" should be criterion for Federal Funding.

SOME GENERAL CRITIQUES OF ONE SYSTEM:
PROPOSAL PEER REVIEW

I was, I believe, one of the first, in testimony before this committee (1975) to make the point that there is an enormously wide spectrum of procedures used by different agencies and within one agency, all of which are called peer-review. Obviously these criticisms will apply in greater or lesser degree in a particular system.

Within the last few months I have published two major articles which critique the present proposal peer review system in detail (Roy, 1985a,b). Rather than repeat the arguments I append them to this testimony. I note, in passing, that my experience is largely with the physical sciences and engineering. A summary of the weaknesses of the present system are as follows:

1. It is extremely wasteful (25-33%) of the commodity in shortest and finite supply--the time of the (best) engnieers and scientists.

2. The length of funding of most projects is ludicrously short making for both inefficiency (in repeating the process) and wastefulness (one cannot make any babies in four months!!).

3. It is inherently and inescapably strongly biased against genuine innovation.

4. It is riddled with conflict of interest, with little or nor attempt to avoid it (except very recently and very timidly).

5. It does not define "the best science," "a peer," "the rating scale," etc.

6. It is too tedious and slow (taking 6-18 months) for the best scientists to fool with.

7. Every major announcement of some Peer-Reviewed New Funding "Initiative" sets the nation back substantially. Literally dozens of person-years are removed from productive research to writing proposals (for data on the setback to science see my example 1985(a): the DoD Equipment Grants. Recently the NSF program for ERC's received $2.2 billion worth of proposals and funded only $10 million. It is easy to calculate the negative advance of U.S. Science by this procedure, towards an exceedingly important goal. "Strong managers" from the agencies could have identified the 25-30 strongest candidate institutions and site visited them with a 6-person team and made the decision in 3 months.

It is ghastly parody of "fairness" to pretend that stimulating the generation of a number of proposals which is 30 to 50 times the number of awards is anything other than an insane process. Yet may I be permitted again, with absolutely no malice, to show how badly our values are skewed when the Science Advisor to the President regards this as evidence of successs or health in the system.

> "We had $10 million in NSF's 1983 budget for the centers, and we had proposals [submitted] totaling $2.2 billion. That kind of response with new proposals in a matter of months does not occur unless those universities had ideas

for iton their own, so that when the Federal program was in place they were ready to jump."

Physics Today, Feb. '86

Mr. Keyworth (who never wrote such proposals in his life) didn't even realize we in the Universities are totally geared up to produce proposals as your own Committee document more wisely hints. The goals of engineering and science become distorted to getting the money first, only secondarily to doing the research.

RECOMMENDATIONS FOR IMPROVING PRESENT SITUATION IN PROJECT SELECTION

I. Congress much take a broad look at the relative dollar allocations between

(a)
- Basic research purely for cultural ends and the good of science
- Basic research connected directly to public or private sector objectives.

(In my view the tilt towards the former must be removed from the system.)

(b)
- Basic research which feeds out into the international science pool.
- Applied research which connects this to useful products, devices, etc.
- Engineering research.

(In my view there needs to be much more of the latter two categories.)

(c) Public-supported Basic Science is easy to define: The closer to human need the more basic the science. In the light of such a definition the balance between disciplines, and between science and engineering.

(d) The ratio of Research oriented toward defense and towards U.S. industrial needs: Many studies show that clear correlation between a decline in industrial competitiveness and defense spending. National security is now more threatened by industrial weakness.

II. Immediate reasonable-scale experiments should be mandated within every agency to evaluate new selection procedures. Unless we try new approaches the U.S. will go down carrying the flag of an outdated proposal peer review system.

Among these, two alternatives are recommended:

For the most basic, innovative, research: The Peer-Reviewed Productivity-based Formula (PRPF) system.* In several presentations of this theme at national meetings over the last several years I have yet to receive a single major criticism. On the positive reinforcement side, I note that the Packard Bromley Committee tacitly agree with my basic thrust that the "track record of the investigator" be made much more important in judging.

The formula has overwhelming advantages:

1. Efficiency--it will save 25% of the time of our best people.

2. Opening up the possibility for genuine, totally unconstrained, innovation by the individual.

3. It will restore science--in part--to the exciting personal adventure it once was.

4. Fairness, and greater evenness in distribution. Many schools which get zero will get something.

5. No sharp ups and downs which are terribly wasteful.

6. Guaranteed fairness, no conflicts of interest.

7. Easy policy making by Congress, by changing the weighting coefficients.

It has one major disadvantage: The bureaucracy for administering the system shrinks by a factor of 10.

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*See next page.

PEER-REVIEWED PRODUCTIVITY-BASED FORMULA FOR FUNDING UNIVERSITY RESEARCH

(See Rustum Roy, Science, Technology and Human Values 10, 73-81 (1985).

The Peer-Reviewed-Productivity Formula (PRPF) System

Let me now turn to a description of my alternative system for funding basic research. The "theory" supporting this approach starts with accountability to the public that pays the bill. The analysis begins by answering the economists' question: Why the public should fund such research with no specific missions or goals. What are the products of such research at a university? I identify four such types of products.

- (a) New knowledge made available to the world at large via published research.
- (b) Students trained at an advanced level (MS and Ph.D. principally) by apprenticeship in the research process, who become the staff of the entire national technical effort.
- (c) Establishment of a research capability—in terms of people, new knowledge, research instrumentation, etc.—enabling the university to participate in the more telestic (i.e., with purpose, targeted, mission-oriented) research funded by mission agencies like DOE, DOT, OSHA, and DOC).
- (d) Potential for a university to conduct research in support of (i.e., directly coupled to) the private industrial sector which provides a major part of the employment and economic base of the society which funds the research.

These are the major, quantifiable benefits society can expect from its atelestic research involvement.

In keeping with the view that the best predictor of success is the past track record, the rationale for this system then says that funding should be *proportional* to the past productivity (i.e., the benefits returned to society) of the research group or researcher in the aggregate of the four areas enumerated above. In practice, as in other institutional funding schemes, the government grants are *not* to individuals but to a group typically of the size of a department or interdisciplinary laboratory. (See below for schemes for funds allocation *within* the group and for arrangements for young investigators.)

The system would operate as follows: The agency division—e.g., in chemistry—would maintain records submitted annually by every institution of higher education conducting research and/or graduate education. The data supplied would include the following information on the "productivity" in each of the main categories enumerated above.

- (a) The number of papers published mainly in an agreed-upon set of journals by the faculty in the unit (department or laboratory). Each faculty member can only be counted as one, although her/his work may be split over two or more units, with no double counting of product permitted. At a later stage, citation indices may be used, but certainly for the first stage, as Roy, Roy and Johnson[11] have separately shown, there is little difference between papers and citations as productivity measures when one aggregates 20 to 40 faculty over three to five years.
- (b) The numbers of M.S. and Ph.D. degrees granted, in each unit, each year. A simple weighting scheme of 3 × Ph.D.'s + 1 × M.S.'s will reflect fairly on the relative effort if not the value of this "product."
- (c) The appropriate measure of how well the particular research capability serves national needs is the amount of research support obtained from mission agencies. This dollar amount is the aggregate research support from all mission agencies for the faculty in this unit, and it measures the unit's utility to the public sector.
- (d) The final term is analogously measured rather easily by aggregating the total support for research (not gifts) received from industry and quantifies the unit's value to the private sector.

These data have been easily available for several years. The terms actually used would be a rolling average of the last three (or four) years. A great advantage of the system is that no step-function changes up or down are possible, preserving the continuity essential in long-term basic research.

The Combined Formula

The actual formula, based on the data provided, would provide for a weighting of each of the four terms as follows (where all data are rolling three-year averages):

Total $ to unit
- = A × # of Publications
- + B × (# of M.S.'s + 3 × # of Ph.D.'s)
- + C × $ vol. of research support from mission agencies
- + D × $ vol. of research support from industry.

The weighting factors A, B, C, D, can be adjusted by the agency or by the Congress to fit the total budget. Obviously, these weighting factors would be adjusted to fit the characteristics of the field; where there is little or no mission agency or industrial support, the factors C and D could be zero. The *relative values of these terms* becomes a simple adjustable policymaking tool. It is easily understood by agency heads and Congresspersons. For example, if a member of Congress wants to encourage university-industry coupling, then no one needs elaborate schemes drawn up by bureaucrats far from the action, which cannot possibly be optimized for different disciplines, different industries, and different institutions. All Congress has to do is to raise sharply the weighting factor D in a particular field, and the signal of this incentive immediately goes out throughout the land. Likewise, if personnel shortages are acute *in one area* and there is a surplus in another, Congress simply rules that, for example, the factor B will be tripled in engineering and halved in theoretical chemistry or whatever.

For more targeted and group research: The strong manager system should be used much more widely. Just as in DoD the manager is a member of the research community he/she funds; but he/she is also closely coupled to the *long-term* research needs of the mission. NSF should try this system in *all* group or collective proposals. Fanning the flames of proposal writing is on a par with starting forest fires--it should be outlawed.

III. *Immediate improvements in the present peer-review system should be required mandated*, with each Agency *required* to hold hearings and respond with the most creative changes proposed by the community.

(a) It is universally agreed that the review should be of the P.I.'s recent research output *not* mainly the shortened (2-3 page) statement of *general* directions of proposed research.

(b) The set of reviewers must represent a *national cross-section of professionals*, i.e. it should *be mandated that* on the average, *if 80%* of the Ph.D.'s in materials science work in industry or government, then *80% of the reviewers* of publicly supported research *must come from the groups*.

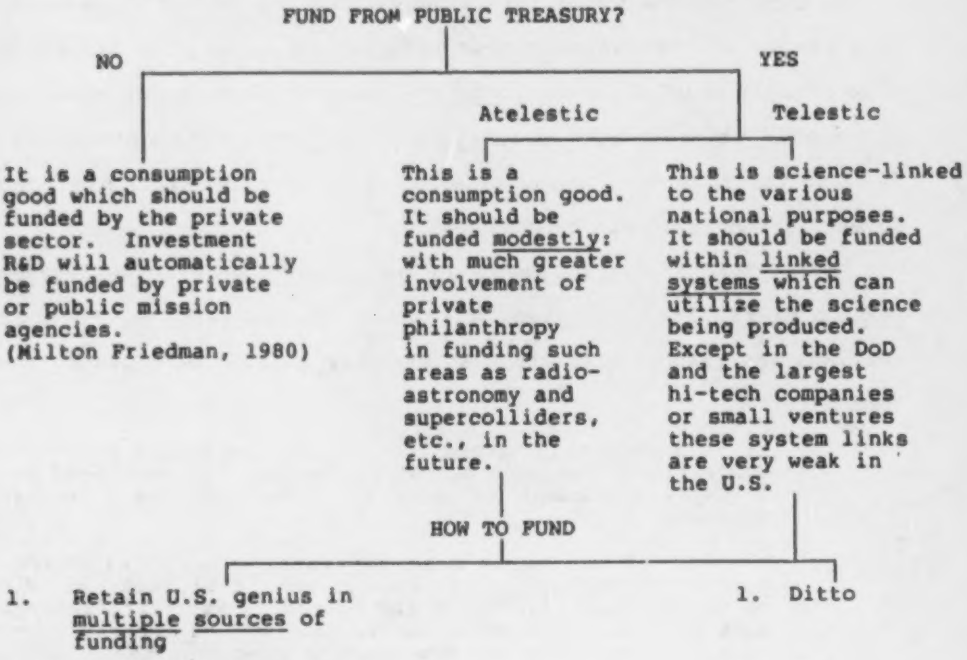
(c) The set of reviewers must include--following Alvin Weinberg (1963)--one or two from outside the speciality field. The value of good work is how it will affect neighboring fields.

(d) The process should be complete in 6 months maximum.

(e) The projects should be funded for a minimum of 3 years.

(f) In University-Industry coupled programs, external peer review is absurd. The best peer review is provided by 'n' companies putting down $30,000 each for a research program spelled out by the P.I. The agency role should be simply to match it as long as it is within the scope of its program, and the results are publicly disseminated.

A General Algorithm for Research Project Selection



FUND FROM PUBLIC TREASURY?

NO	YES: Atelestic	YES: Telestic
It is a consumption good which should be funded by the private sector. Investment R&D will automatically be funded by private or public mission agencies. (Milton Friedman, 1980)	This is a consumption good. It should be funded modestly: with much greater involvement of private philanthropy in funding such areas as radio-astronomy and supercolliders, etc., in the future.	This is science-linked to the various national purposes. It should be funded within linked systems which can utilize the science being produced. Except in the DoD and the largest hi-tech companies or small ventures these system links are very weak in the U.S.

HOW TO FUND

1.	Retain U.S. genius in multiple sources of funding		1. Ditto
2.	Fund the person on the basis of cumulative recent track record (Roy's peer-reviewed productivity based formula) or by strong manager. Eliminate all mail-evaluation of "essays."		2. Ditto
3.	All "Big Science" to be internationalized: no more competing accelerators and phased arrays of radio telescopes.	Very major increase in modest size group science linked to private sector or public sector at cost of both Big and Little Science.	Couple individual (telestic) science to national industry or government objectives.

4. Different fields of engineering/science will use a different mix of Big Group and Individual, science. Basic atelestic science should not be evaluated by criteria appropriate for engineering, but nor should the opposite be permitted any longer.

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ALTERNATIVES TO REVIEW BY PEERS: A CONTRIBUTION TO THE THEORY OF SCIENTIFIC CHOICE

By
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Alternatives to Review by Peers: A Contribution to the Theory of Scientific Choice

RUSTUM ROY

IN THE mid-1960s a series of papers by Michael Polanyi, Dr Alvin Weinberg and others opened the discussion of "scientific choice".[1] The term "scientific choice" referred to two rather disparate choices. The first was the choice among subfields of science with respect to allocation of resources. The second was the choice among different possible performers of research as to which should be supported. Since those happy days for science, the former question, possibly the most critical issue of science policy, has virtually disappeared from the range of concerns of the makers of science policy. In 1977, in the wake of President Carter's push for "zero-based budgeting", whereby the allocations made for the next budgetary period would disregard the pattern of allocation of the previous budgetary period, I suggested to Dr Philip Handler, then president of the United States National Academy of Sciences, that the Academy create a committee to reflect on the desirability of a "zero-based budget" for science and engineering. Dr Handler's response was that such a task was impossible since it would arouse bitter conflicts among scientists. Under present practices, the support of American science research is frozen into a more or less fixed pattern of distribution of support for engineering, applied scientific research and pure scientific research, and among different subfields; if the pattern were once appropriate, it appears less and less so every day.[2]

The issues raised in the discussion of scientific choice reappeared in the 1970s in connection with "peer review". Unfortunately, the entire focus of the argument was shifted from the important problem of the best way to distribute the total funds for the support of research, to an analysis of one of the less significant methods of deciding how funds should be allocated. The persons who discussed "scientific choice" seemed to take for granted that allocations would be decided on the basis of review by peers. Some of the criticisms focused on the possible miscarriage of justice to individuals. The debate about the merits and defects of review by peers diverted attention from the more important issue of what different systems are available for the distribution of research funds.

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[1] Polanyi, Michael, "The Republic of Science: Its Political and Economic Theory", *Minerva*, I (Autumn 1962), pp. 54–73; Weinberg, Alvin M., "Criteria for Scientific Choice", *Minerva*, I (Winter 1963), pp. 159–171, and "Criteria for Scientific Choice II: The Two Cultures", *Minerva*, III (Autumn 1964), pp. 3–14; Maddox, John, "Choice and the Scientific Community," *Minerva*, II (Winter 1964), pp. 141–159; Carter, C. F., "The Distribution of Scientific Effort", *Minerva*, I (Winter 1963), pp. 172–190; Toulmin, Stephen, "The Complexity of Scientific Choice: A Stocktaking", *Minerva*, II (Spring 1964), pp. 343–359.

[2] Shapley, Deborah and Roy, Rustum, *Lost at the Frontier: American Science and Technology Policy Adrift* (Philadelphia: ISI Press, 1984).

Defects of the Present System

The many government agencies which use the system of review of proposals by peers, and which have sponsored many attempts to validate their system,[3] have not supported a single study to compare such review with other systems of allocating funds for research by scientists of similar qualifications; there has been no comparison with the "strong-manager" method used by the United States Department of Defense, or with the formula system. No effort has been made to examine the "efficiency" of the system in terms of the costs and time required for each grant, or the efficacy of the system in supporting genuine innovation.[4]

Even without systematically analysed comparative data, the failures of review by peers as a way of deciding which projects and which scientists should receive grants seem to be very evident. Yet virtually no senior official has commented on the glaring deficiencies of these procedures. Let us examine a recent example. In 1983 the Department of Defense started a new programme making available $30,000,000 annually to provide some large items of research equipment to universities. The Department of Defense deviated from the procedures used by many of its own subdivisions, which could presumably have selected for those universities working with the Department the articles of equipment most needed. Instead, in an effort to gain public favour, it issued an invitation for proposals to all universities, whether or not there were research groups with significant support from the Department of Defense in those universities. This resulted in a fiasco. Over 2,200 proposals were received for a sum of $625 million. The success ratio was less than 1:60. The time required for the preparation and submission of each proposal may be estimated at one month's work of one person. Thus, 2,200 scientists spent one month—or nearly 200 years of scientific work were diverted from research. Fortunately, scientific peers were not used to evaluate the process; but any estimate of the total expenditure of time must come to perhaps one more year of one scientist's work for each grant made. If we include an average figure of 100 per cent for overhead costs, the cost therefore was equal to 400 average salaries at $40,000 per scientist per year, and the allocation of $30,000,000 cost about $16,000,000. This example does not include the administrative costs of a typical review by post, or of panels of peers. The entire operation, moreover, further exacerbated the differences between the successful and the unsuccessful applicants, since the unsuccessful universities wasted their efforts, while those with the more successful applicants gained even more.

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[3] Cole, Stephen and Cole, Jonathan, "The Ortega Hypothesis: Criterion Analysis suggests that only a Few Scientists contribute to Scientific Progress", *Science*, CLXXVIII (27 October, 1972), pp. 368–374; Cole, S. and Cole, J. R. and Simon, G. A., "Chance and Consensus in Peer Review", *Science*, CCXIV (20 November, 1981), pp. 187–255.

[4] Roy, Rustum, "Peer Review of Proposals: Rationale, Practice and Performance", *Bulletin of Science, Technology and Society*, II, 5 (1982), pp. 405–418; Roy, Rustum, Testimony at Hearings on "NSF Peer Review" before the US House of Representatives Subcommittee on Science and Technology, No. 32 (29 July, 1975), pp. 684–693.

The failure of the system of review by peers is not that it fails to provide financial support for most good scientists. No system could fail to do that! The unnecessary waste of limited resources of scientific talent is the single most telling failure of the peer-review system.

There is no single system of review by peers. There is an infinity of systems in which the influence of the scientific "peer" varies from almost nothing more than the passive lending of a name for legitimation, to almost complete control. I will consider only four major categories of review by peers: reviews submitted by peers, plus an assessment by an assembled panel, plus a site visit; reviews submitted by post, plus an assembled panel; standardised postal reviews, usually with quantified ratings or preferences within prescribed categories; and postal advice or comments without ratings.

Only the most sanguine advocate unfamiliar with the literature would claim that there is any basis for expecting a correlation between a scientist's ability to present an essay and the actual future production of the "best science".[5] The weak links in a "theoretical" sense are that we have no definition of what constitutes the "best science". With the total confusion between the terms "basic" and "applied" and over the value of relevance, and the very major psychosocial differences in perceptions and values, between—let us say—civil engineers and theoretical physicists, the entire system of review by peers is one of reinforcements of the idiosyncracies or the ruling paradigms of any group which is constituted and supported as a unit.

We have no definition of a peer. According to current practice—strongly departing from the judicial model—a peer is defined as one who works in the same narrow subspeciality of scientific research. For example, proposals in microwave plasma synthesis exclude those working in radio-frequency plasmas, or those working in chemical vapour deposition synthesis. The working definition of peers in the present peer-review system certainly means as narrow a group of specialists as can be found to match the subject defined in the proposal.

But is not a better definition of a peer, a person of equal "rank" and "experience" in science, drawn not only from the narrow speciality, but explicitly including the neighbouring fields? Dr Weinberg's insight that the best science would be that which affected a wide group of fields indicates that peers be explicitly defined to include some from neighbouring fields. Not a single agency of the United States government does this.

In the present system, the simplest precautions against conflict of interest are ignored. The system flies in the face of the most elementary knowledge of human nature and presupposes a level of objectivity, disinterestedness and honesty, such as never obtained in any human group. Proposals, possibly with the literature thoroughly surveyed, the investigator's best ideas clearly expounded, and experiments specifically laid out, are sent to the set of colleagues who can most adequately evaluate the proposal but who also could use this same information in their own research. Moreover, in the present climate of opinion, a colleague who knows that he or she has the certain power to doom that proposal by a check mark in the "Fair" or "Good" category—even if accompanied, albeit inconsistently, by written praise—might well be inclined to use it. Both applicants and assessors know that this could give the reviewer enough time to perform the same or similar research. Applicants in rapidly developing fields, therefore, often employ the stratagem of applying for funds only for work that is already complete and is nearing submission for publication.

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[5] Peters, D. P. and Ceci, S. J., "Peer Review Practice of Psychological Journals", *The Behavioral and Brain Sciences*, V, 2 (1982), pp. 187–255; Harnad, Stephen, "Peer Commentary on Peer Review"; followed by 56 comments, *ibid.*, pp. 185–186, 196–225.

The system of peer review ignores the crucial role of "change" and serendipity in science. Against all historical evidence, the system is based on the idea that genuine discovery can be planned in advance in an essay which meets with the approval of distant colleagues. Obviously, some outstanding work is done by persons who receive grants through peer review of their applications—after all, any system will deliver a good fraction of its funds to the best scientists. This is no vindication of the system.

The defects of the system of review by peers may be summarised as follows: It disregards the multiplicity of systems of assessment and the possibility of combining their best features. It involves an enormous waste of the finite resource of the time of scientists and is inherently unfavourable to innovation. The schedule inherent in the process—often requiring some months to write an elaborate proposal and a waiting period of between six and twelve months—does not correspond with the actual schedule according to which creative scientists work, where the period from gestation of an idea to trying it out is much shorter. The intellectual momentum is thus often lost. The process encourages "competition" instead of co-operation and collaboration as the most effective mode of achieving the best scientific results.

Alternative Systems of Allocating Funds for Research

The only alternatives to the system of review by peers are not lotteries or the granting of equal amounts of financial support to every "qualified" scientist, although such experiments may be worthwhile. Scientists do not seem to wish to consider the more serious alternatives. Scientists who defend the status quo say that the peer-review system is the source of the success of American scientists, as demonstrated by such achievements as the winning of Nobel prizes.[6] Many scientists who are reasonably well supported find it difficult even to consider alternative systems in an objective and open manner. For those entering research after 1950, the system of peer review has become synonymous with support for their own scientific research. Some regard any criticism as a threat to the continued support of their research—hence there has been no scientific inquiry into or discussion about alternative systems. The National Academy of Sciences which has often risen to the defence of the system of peer review has never considered the possible alternatives.

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[6] Committee of Scientific Society Presidents, Testimony at Hearings on "NSF Peer Review" before the US House of Representatives, Subcommittee on Science and Technology (July 1975), p. 1,096; also pp. 1,081, 1,088.

The fact is that scientific research has been supported by an enormous variety of institutional arrangements all over the world; the system of peer review is only one of these. Alternatives do not need to be invented—they already exist in abundance.

In most countries, the existing systems tend to make "block" grants to universities, laboratories or departments. In Great Britain, for example, the University Grants Committee provides grants to run over a number of years for universities; these include sums for the maintenance of departmental research activities such as costs of new equipment and salaries for research. Graduate students are supported again from block grants to departments, from the Science and Engineering Research Council. Finally, the latter body—and other ministries—also makes grants for special research projects on the basis of one-page general proposals. In Japan "laboratory-sized groups" under a senior professor, with some younger teachers and students, are supported for five- to ten-year periods, on the basis of site-visits and review, while students are supported directly by the Ministry of Education. In South Africa, an interesting variant of review by peers focuses exclusively on the most recent research by the individual professor, without requiring any proposal describing the research which the professor intends to carry out in the immediate future.

In the United States itself much more money for research is actually distributed by the "strong-manager" system used by the Department of Defense and other agencies which support "mission-oriented" basic research than by the system of review by peers. In the academic world of the United States, a system for the support of research—review by peers—became firmly established as a result of the involvement of the major universities in military research during the Second World War: with the best of intentions, it turned out to favour those universities. In some disciplines—notably the applied sciences—scientists frequently receive part of the support for their research through the "strong-manager" system and part from agencies which depend on review by peers. Such persons are particularly well placed to compare the systems. Interestingly enough, the fields which are further removed from application, such as theoretical physics and chemistry, radio-astronomy, and many parts of the life sciences, have never experienced any system of support other than the peer reviews employed by the National Institutes of Health and the National Science Foundation. Nevertheless, many leading scientists of the United States are beginning to be exasperated by the waste of time required by the system of review by peers.

Three alternatives to that system are worthy of consideration. They differ in the extent to which they diverge from the present procedures. All three can be tried simultaneously within a single major grant-awarding body. These alternatives share certain major principles or presuppositions. First, past success is the best basis for the prediction of future performance. Second, the support of small groups or individuals on a continuing basis for the appropriate length of time—let us say, for seven years—increases the probability of success and the efficiency of the system. Third, the most innovative science is done in the context of attaining a broadly defined objective, yet carried out with the minimum of close supervision of the goals, methods or budgetary categories of individual projects. Fourth, co-operation between the very best specialists and the bringing together of their very best experimental capacities is a necessary condition for the advancement of knowledge in fields at the frontier of development. The failure of the universities to make institutional provision for interdisciplinary research is, therefore, seriously damaging to them.[7] Fifth, the advancement of scientific knowledge which goes hand in hand with the advancement of the public interest is the most valuable.

All the systems for the allocation of funds for research must reconcile two apparently contradictory requirements: greater freedom for the investigator and greater accountability, not merely for honesty in expenditures but for the goals of research.

A Formula for Support based on Productivity assessed by Peers

Why should the public support research with no specific mission or goal? Such research carried on at universities has the several functions. It makes new knowledge available to the world at large by publication. It trains students at an advanced level by apprenticeship in research. It establishes a capacity for research—including trained scientists, advanced knowledge and scientific instruments—enabling the university to participate in the more telestic research, i.e., research linked to a purpose or mission and supported by agencies such as the Department of Energy, the Department of Transportation, the Occupational Safety and Health Administration and the Department of Defense. It makes possible research done in support of the private industrial firms which produce a major part of the wealth which goes to the support of research.

In keeping with the view that the best guide to the prediction of success is past achievement, financial support should be proportional to the past productivity of the scientist in these four functions. The grants should not be to individuals but to a group, usually of the size of a department or an interdisciplinary laboratory. The arrangement would work as follows: a governmental body concerned, for example, with chemistry, would maintain records submitted annually by every institution of higher education conducting research or graduate education or both. The data supplied would include information on the "productivity" of the institution with respect to the four functions, including the number of papers published, mainly in a set of journals agreed by the staff of the department or laboratory. Each member can only be counted as one, although his or her work may be split over two or more units, with no double counting of product permitted. At a later stage, citations might be used as a measure of scientific productivity, but for the first stage, there is little difference between using papers and using citations as a measure of productivity when one aggregates the publications of 20 to 40 persons over three to five years.[8] The university would provide the numbers of advanced degrees granted, in each unit, each year. A simple scheme of weighting different degrees would reflect the amount of effort used to produce them. The measure of the value of the particular scientific capacity of the unit for serving practical or mission-oriented ends is the amount of financial support received from mission-oriented government agencies. The effectiveness in the performance of the fourth is the total financial support for research received from private industry. These data would of course be available for each year; the measures used would be a rolling average of the preceding three or four years.

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[7] Roy, Rustum, "Interdisciplinary Science on Campus: The Elusive Dream", *Chemical Engineering News*, LV (29 August, 1977), pp. 28–40.

The combined formula: The actual formula using the data provided would be as follows, where all numbers represent rolling three-year averages:

> Total sum to be granted to unit = A × number of publications + B × weighted number of advanced degrees + C × sum received for research from mission-oriented agencies + D × sum received for research from private industry.

The weighting factors—A, B, C, D—would be adjusted by each agency so that the total of money distributed to all institutions would equal the total budget. The relative values of these factors becomes a flexible device for making policy. The weighting scheme is easily understood by high administrators and legislators. For example, if it were desired to encourage collaboration between university and industry, it would only be necessary for the legislative body to increase the weighting of D; this signal would immediately be perceived and acted upon throughout the country. Likewise, if there were shortages of qualified persons in one area, and a surplus in another, the legislature would simply rule that, for example, factor B would be tripled in the field of shortage and halved in the field in which there was a surplus of trained persons.

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[8] Roy, Rustum, Roy, N. R., and Johnson, G. G., "Approximating Total Citation Counts from First Author Counts and from Total Papers". *Scientometrics*, V. 2 (1983), pp. 117–124.

Allocation of academic unit's research allotment to individuals from grants to unit: One of the immediate objections to this scheme by productive scientists is the concern that it will reward the lazy members of the unit. This must be guarded against and it is simple to do so. Many American universities already do what is proposed here in the reallocation of the "return of overhead" to departments and research units. Each unit will be required by the granting body to provide an "acceptable plan" so that individual scientists will benefit from their own productivity. Typically, such a plan might propose that 5 to 10 per cent of the funds will be retained at the college level for major equipment or projects for which any unit or individual can compete. Similarly 10 to 15 per cent of the funds might be retained at the departmental or unit level for commonly used equipment and technicians, etc. The 75 to 85 per cent would then be divided up within the department using the same—or slightly modified—formula so that the most "productive" members of the department would receive the greater part of this genuinely "unearmarked" support.

Summary: This "formula" is, of course, very different from the many other such schemes proposed, in that it takes account of "productivity" in the field in which support is given, and in that it is flexible. Representative George Miller of California, chairman of the House of Representatives Subcommittee on Research, introduced his "formula funding" bill in 1968; his formula did not involve any measures of productivity at all.[9] In contrast, my formula draws on the most intensive and defensible review by peers and hence should immediately command the support of all those who believe in assessment by scientific peers. Review by peers is present through the fact that the vast majority of the publications occur in journals in which the referees are scientific peers. A published paper has undergone peer review which has assessed the quality of completed research. Although peer review of completed research has itself been attacked,[10] it is less vulnerable to criticism than the system of review by peers of proposed research projects. In any case, counting all publications instead of publications reviewed by peers in technical fields would make hardly any difference in the distribution over the country as a whole. The award of advanced degrees, except in some master's degrees, has been subject almost always to collective judgement by the department or the committee of examiners. A successful application for grants for research from "strong managers" in a mission-oriented agency is also conducted after a much more stringent review by highly qualified peers than any postal review of a proposal. Finally, by far the most stringent review by a peer is that which occurs when a research-manager in an industrial laboratory allocates $50,000 to support research at a university.

The formula which I propose here can deal with the situation of the new, usually young member of the unit in several ways. Any unit which appoints new members should receive one extra share for each new member equal to the average allocation for research for each member of the unit. If the newly appointed person is a replacement, there is presumably already enough money to pay the salary and the expenses of the research. This averaging scheme in my view is less perturbing to the pattern of any particular university than a fixed award for young scientists. An award of $50,000 per year for a new assistant professor who has recently been awarded the doctorate might not be unreasonable at the major universities, but it could cause jealousy at some others.

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[9] Representative George P. Miller, author of HR 35, Testimony at Hearings before US House of Representatives, 91st Session of Congress (February 1969).

[10] Peters, D. P. and Ceci, S. J., *op. cit.*; Harnad, S., *op. cit.*

A comparison of the scheme I propose with the existing system of awards on the basis of review by peers shows that the proposed scheme rests on a clear principle. It is a proportional award by the representatives of society for delivering the "products" those representatives desire. Moreover, it is based on recorded, quantifiable performance, not on promises made in an essay. The "best science" is defined as that which honours both scholarship and the public interest, and is "quantified" in the four terms of the formula. There are, moreover, different sets of peers for different evaluations. In most of these, the particular group of peers is much more broadly cognisant of other fields of the national interest, or industry's wants, than in the prevailing system of peer review. In addition, conflict of interest is virtually eliminated. Whereas in the system of peer review, a single negative review by one of the peers can block the entire research of a senior scientist with less than six months' notice, no single person can affect the immediate future of any individual drastically.

This elimination of conflict of interest goes far to remove the corrosion of the integrity of the community caused by the present system. It goes much further than the commendable though mild effort by the National Institutes of Health in the warning statement which its reviewers are asked to note, but not even to sign and return. Because the recipient is not constrained by any proposal, or in any fashion in the National Science Foundation, the National Institutes of Health or among his colleagues, he can follow any unforeseen lead or any unexpected opening. He can chose to do the most risky experiments in his genuinely atelistic work, i.e., work without a particular goal in view, by balancing it with work for a mission-oriented agency and industry. Dr Weinberg's proposal made 20 years ago that atelestic research should be an "overhead" on mission-oriented research will have been realised.[11]

The magnitude of an individual's research grants will vary gradually over time, guaranteeing every scientist a certain proportion of the stable allocation of funds to do some of the difficult work requiring five or more years, which is now shunned for the quick production of papers. The beneficial feature of the formula is that it provides no sharp line of demarcation with respect to quality of individuals or universities. Thus it corresponds much more accurately to the realities of the distribution of talents. This schema has the merit of rewarding productivity of the same "commodities" wherever they are produced. A wider range of universities will get some money on a steady basis, even if the total sum is not large.

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[11] Weinberg, A. M., *op. cit.*, 1963, 1964.

It is not by any means obvious that this funding scheme will by itself increase co-operation. It will, on the other hand, certainly diminish dishonesty, which as Dr Yalow has pointed out may be as harmful as anything else to the scientific community. It will make co-operation a little more likely.[12]

A Simpler Formula for Matching Support

A simpler version of the formula proposed above has been advocated in the various schemes proposed by others for "matching" funds. Thus, Dr Weinberg's idea would provide basic research funds to a unit or an individual in direct proportion to the mission-oriented research done by that unit or individual; in the formula which I propose it would consist only of the third term. Similarly, the report of the commission on industrial innovation, under the chairmanship of Jordan Baruch and appointed by President Carter, proposed that universities should receive five to seven times the amount provided by industry.[13] In effect, this proposal is represented by the fourth term of my formula. I believe a case can be made for using Dr Weinberg's rationale and reducing the formulae to a matching grant proportional to C times total support by mission-oriented agencies plus D times support by industry. This would, however, limit the general applicability of the formula since certain less applied fields would be at a severe disadvantage.

Support Based on Peer-Evaluation of Performer's Past Achievements

The system based on performance can be applied to individuals. In this variant, support would be provided on the basis of the entire, fairly recent achievement in research of the individual. This is the system now being used by the Council for Scientific and Industrial Research of South Africa.

The peers who are selected to evaluate the scientific achievement of any individual must rigorously exclude any possible conflict of interest. In the case of American academic scientists, this would exclude other American academic scientists. Industry and government scientists who cannot possibly be supported from the same source provide a much larger pool of candidates who are less likely to be biased. The reviewing group should be drawn by random-number selection by computer from a qualified set selected for each subdiscipline, and should include one or two from related disciplines. The qualified sets of reviewers would be revised annually by a committee of the National Research Council. In general, it might be valuable to have small groups of individuals in roughly the same field all evaluated by the same group; this would provide a basis for comparative judgements. The best group of peers is obvious: it is the group of scientists in the same general fields in the many similar institutions in foreign countries.

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[12] Yalow, Rosalyn S., "Is Subterfuge Consistent with Good Science", *Bulletin of Science, Technology and Society*, II, 5 (1982), pp. 401–404.

[13] Baruch, Jordan, *Final Report of the Advisory Committee on Industrial Innovation, United States Department of Commerce* (Washington, DC: US Government Printing Office, September 1979).

The data presented to the assessors would be a curriculum vitae, up-to-date bibliography, and perhaps a two-page statement by the individual being assessed summarising his or her recent achievements in research. Again, this kind of judgement is hardly alien to academic traditions. It is made daily in appointive decisions in universities and industry. The peers would rank their evaluation of the quality, quantity and the originality of the research and its value to society. The actual budget allocations could be left to the managers since this would be dependent on the subfield of study.

International Evaluation by Peers of Established Groups

This is a variant of the scheme for individuals applied to departments or other research units. It is in widespread use now in many parts of the world. In its ideal form, unfortunately, it cannot cope with large numbers of institutions. For example, to allocate funds among half a dozen bioengineering groups, each group would prepare a statement of recent achievements and provide the data on its productivity over the preceding five years. The international group of assessors would both evaluate each group's "productivity", compare the productivity of the groups in the set, and point out possible overlaps, gaps, etc. Ideally, the peer group would visit each of the units, assess its capacity "on the ground" and provide a much more precise recommendation regarding the relative merits of each unit working on the subject to the grant-awarding body. Whenever such assessments have been used, they have been regarded as eminently fair.

Clearly it is impractical to have the same group of assessors visit 100 or even 25 institutions. A postal review alone could perhaps be done for 20 or 25 by the same set of assessors, but several sets of assessors may be needed when large sets of institutions must be evaluated. Visits to sites by an international peer group are clearly the best way to evaluate the smaller sets of specialised research institutes, such as water research institutes and materials research laboratories. It is again a mark of the conservatism of the management of American research institutions that this has never been done. It is a pity, since the international nature of the reviewers would very properly reflect the realities of modern science.

An Optimal Review of Proposals by Peers

Since the smaller the change the more likely its adoption, I include for completeness an alternative to the present system, modifying it in simple ways which would eliminate some of the major objections to the present system and which would not be more costly.

From the outset, it should be made clear that the system seeks to evaluate the proposer and the ideas, and that each will be weighted equally or that greater emphasis will be laid on the former. Hence, the proposal should present in detail the recent research achievements of the programme and provide a short general indication of the work to be attempted. The review form must explicitly explain the two different evaluations being requested.

Two simple sets of precautions will avoid conflict of interest by selection of assessors from industry and government, as I have already recommended, for all academic applicants. One can very easily avoid manipulation of the selection of referees by the manager of the programme by using a randomised selection from a qualified set of referees designated by the National Research Council or a similar body. A simple legislative enactment would solve this problem once and for all. For judgements regarding the allocation of public funds, the proportion of peers drawn from industry, governmental and academic laboratories should reflect roughly the national percentages of working scientists in the particular discipline throughout the country.

The instructions to assessors should be very clear—as they are not today. They should be designed by experts in the preparation of questionnaires. The instructions should include the probabilities of the outcome associated with each level of rating. Thus the assessor should be informed that, say, an average rating of B− results only in a 5 per cent probability of support, A− in 75 per cent and an A+ in 95 per cent probability of support. Similarly the exact weighting to be placed on the applicant's achievements in research and on his proposed research should be indicated unambiguously.

Once the assessments are received, they should be sent to the applicant for technical rebuttal, to be completed within exactly two weeks and with only one cycle of such rebuttal permitted. This avoids absurd errors in the referees' judgements.[14] The Dutch government uses this system.

It is clear that we have absolutely no guarantee that a "principal investigator" will actually do the research he has proposed and whether it will be "successful", and if so, whether it will be of any value to science. Hence, it is absurd to have a sharp disjunction between successful and unsuccessful applicants in the distribution of funds. The budgets should be reallocated to diffuse the boundary by taking the bottom 15 to 20 per cent of the successful applicants and cutting their budgets by, let us say, 25 per cent and awarding partial support—from 33 to 50 per cent of their requested sum—to 10 to 15 per cent of those just below the present dividing line. Getting something started, or maintaining something already under way, is often invaluable to morale and very economical.

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[14] Recently one of our proposals was classified as border-line and turned down by the National Science Foundation, although one of the reviewers' main reasons for a lower rating was that in an interdisciplinary proposal we had not included Professor X., the best known person at Pennsylvania State University. The reviewer might have had a point had Professor X. not been dead for over two years. The National Science Foundation, while embarrassed, had no way to correct the error because the process by then had moved to the next stage.

In the United States where different major agencies which support research use different systems, it would be a worthwhile experiment to employ both the schemes within the same agency in different divisions. After some years, the parts of the scientific community which have had experience of both schemes could be surveyed for their preference. Meanwhile, studies of the preferences of scientists who have already been supported by both systems might be undertaken immediately to justify the experiment.

Conclusion

The questions of scientific choice which were left unresolved when the rapid expansion of academic science in the United States began in the early 1960s have come back to trouble the scientific community. There is now widespread dissatisfaction with the process of review by peers as one of the major systems for the allocation of public funds for research. While earlier criticisms had been brushed off by the assertion—unsupported by facts—that no other systems existed, the present situation cannot be so easily dismissed.

A serious examination of other national and international arrangements shows that a wide variety of procedures are in use and there is no research which shows that one system is either more productive scientifically, or more cost-effective in bringing about valuable scientific research. New systems which may be considered should avoid the major defects of the system of peer review as now practised: the enormous waste of scientists' time, the great potential for conflicts of interest, and the inherent bias against innovation.

The principal system which I have proposed here combines the best elements of peer review with the simplicity and efficiency of the use of a formula. Moreover, this formula based on peer review of performance incorporates all the elements for which the academic scientific establishment should be accountable to its patron, which is the public treasury. A final virtue of the proposed system is that it provides simple and convenient procedures through the use of numerical weighting factors for the policy-maker to guide the support of scientific research as a whole.

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Funding Science: The *Real* Defects of Peer Review and An Alternative To It

Rustum Roy

The existence of the procedure of peer-review of *proposals* (as distinguished from review of papers or completed works) is an extraordinary phenomenon. It has survived for some 30 years because of three factors. First, the inherent elitism of the hard sciences and its arcane linguistic barriers have enabled some scientists to claim that science should play by special rules. The rules that evolved, without any published analysis or forethought, then became a Kuhnian "ruling paradigm" of the science policy community and have been ritually affirmed as correct by most scientists and policymakers. Second, there is now a generation of scientists who have experienced no other means of obtaining research funding and hence feel their entire research career to be at risk when any questions are raised about the peer review system. Third, the social scientists occasionally commissioned to examine the system have been co-opted by the paradigm of the "hard" science community and have yet to formulate even the most obvious questions that should be asked about any public system for distributing funds. From the perspective of national policy, their research has addressed trivial issues.

In this article, therefore, I will first summarize the major criticisms of the procedures actually used by many proposal peer review systems. I will then show that, contrary to many scientists' assertion that "there are no alternatives," many alternative systems do exist and are in use in this country and abroad. Moreover, such systems disburse at least 90% of the total money spent on research and development in the United States. Finally, I will outline a radically different alternative system for disbursing research funds, one that is a more comprehensively *peer-reviewed system*, but one that involves not *a promise in an essay* (i.e., proposal), but uses a *track record of performance* in research.

My approach to the problem is no doubt influenced by my own experience. As first a physical chemist and now a materials scientist, I have been an active researcher at the same university for 35 years. Since September 1950, I have had continuous research support from a variety of Federal agencies and industry. Since the early 1960s as a senior investigator I have had roughly 6 to 12 research grants simultaneously, totaling between $500,000 and $1,000,000 per year, secured on the basis of dozens of "proposals" of all kinds every year. These facts are relevant to establish two points: (a) I am an active and *current* (many of the *senior* defenders of the system have not had experience with the system in its *present* form) participant in the system, and (b) my opinions are not those of an unfunded outsider rejected by the system.

The Existing System

Origins of the Peer Reviewing of Proposals

It is possible that even while Thomas Kuhn was developing his thesis of the role of the "paradigm" in the development of science, an excellent illustration of such a paradigm was rapidly being established in the field of science policy. Borrowing

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on the tradition of a "trial by jury of one's peers," within a decade (1950–1960) a rigid framework was created for institutionalizing the funding of "basic" scientific research in the United States. No individual or committee report originated this idea. There did exist, of course, a tradition of editors asking referees to review completed manuscripts submitted for publication. But editors of major journals then really ran their own shows; reviewers offered advice, they did not make trial judgments.

In the late 1940s, within the Office of Naval Research (ONR), the first systematically organized government source of research funds for universities, peer review began as an informal "seeking of a second opinion" by the grants manager, who mailed a copy of a proposal on the periphery of his competence to a colleague and followed up with a phone call. When Alan T. Waterman moved from ONR to become the first director of the National Science Foundation, this process became formalized into one of getting written reviews from "peers." It was actually regarded as an expedient (as Philip Handler, President of the U.S. National Academy of Sciences, phrased it), "to harmonize the elitism of science with the political demands of a democracy." In an era of expanding budgets during the 1950's and early 1960s, without any thought, examination, or analysis, each different system (idiosyncratic to each separate agency) became enshrined as "THE peer review" system, the inviolate model for disbursing grant money. An extraordinarily perceptive description of the concept can be found in the etchings of a Soviet artist, Zlatkovsky, taken from the book by the mathematician V.V. Nalimov, entitled *The Faces of Science.*[1]

From those humble beginnings, the "peer review of proposals" became a hydra-headed juggernaut (to mix Greek and Indian metaphors), totally unstoppable even by the thoughtful protests of many senior science policy savants. The matter of "scientific choice"—what science to fund and how—was addressed at length in the early 1960s by Michael Polanyi,[2] Alvin C. Weinberg,[3] John Maddox,[4] C.F. Carter,[5] and Stephen Toulmin.[6] (Bruce L.R. Smith[7] has provided an excellent retrospective summary of their positions.) But it is important to note that none of their analyses, nor the analyses in Congressional testimony by a National Academy Panel[8] that included Edward Teller, Arthur Kantrowitz, Hendrik Bode, and Harvey Brooks, *had any effect whatsoever* on the actual practice of the peer review systems in use at the National Science Foundation and National Institutes of Health.[9]

Some of the Criticisms

For nearly two decades, however, several of the peer review systems in use in the United States have been severely criticized by a vocal but small minority of the academic community, and more frequently by those concerned with science policy. Other members of the community have defended the existing system, although such defense has been general rather than specific.* Reading such criticisms and defense leads to a most remarkable observation: The charges of the critics and the statements of the defenders seldom appear to refer to the same topics.

The charges of the critics of the current peer review system may be summarized as follows.

(i) There is no theoretical or empirical justification to support the contention that "good" research can be predicted on the basis of the "evaluation of" proposed ideas contained in an essay. Indeed, no established criteria exist to judge what is the "best" science and whether the same criteria are useful in applied science and technology. Many claim that the system is strongly biased against the engineering community.

(ii) The system is inordinately wasteful of scientific talent, even the best scientists being forced to spend one-quarter to one-half of their time on securing their funds, rather than doing the research.

(iii) The system is intrinsically inimical to innovation (see Muller[10]); this situation occurs because few leading scientists are willing to expose their best ideas to their competitors, and because it often takes 9 to 18 months to get the funds, and because radically new ideas will always be viewed critically by the majority.

(iv) The system is psychologically naive, especially in times of shrinking budgets, because it is built on patterns of behavior that cannot be expected from humans and hence encourages dishonesty (see Yalow[11]). Because of the absence of even simple procedures to avoid conflict of interest, scientists are often asked to judge the work of competitors, knowing that an excellent rating might jeopardize their own funding.

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* For a decade, from 1971 to 1981, I have written articles, editorials, and letters to the editor, and have testified before Congress, to make the case that the current system is seriously deficient. These defects of the system are described in many other articles of mine and I refer readers to those articles for additional details.[9]

(v) The system is universally acknowledged to be susceptible to manipulation by managers who operate under the emotional cover of the "peer review" rubric so that *individual* (although not the average) proposals may be favored or disfavored simply by appropriate selection of the reviewers.

(vi) Finally, the system has resisted even minor improvements based on suggestions by the community it is supposed to serve.

One might expect that several research projects have addressed these issues. And indeed, there has been a modest amount of research supported on peer review; but these studies have been extremely limited in scope and have addressed a different, almost microscopic aspect of the problem. This issue, which does indeed stimulate much of the criticism, is whether the system has been fair to an individual on a particular proposal. The response to such criticism is in itself remarkable. Statistical studies are carried out on large numbers of proposals to establish that *on the average* the system is fair. One wonders if a plaintiff in a robbery case would be satisfied by a similar statistical argument that, on the average, no one was robbed in New York.

Justifications of the Current System

The National Science Foundation has funded two extensive studies of its system by Cole, Rubin, and Cole[12] and by Cole and Cole.[13] It is instructive to study the prefaces and introductory sections of these documents, for they reveal more clearly than any data the issue at stake. Briefly, both studies of the NSF peer review system ignored *all* the criticisms above and focused instead on the single issue "was the system fair?" The chair of the National Academy of Sciences committee responsible for the study wrote:

> It presents the results of Phase 1 of a study of the peer review system in the National Science Foundation (NSF). By peer review we mean the evaluation of proposals for research in the science disciplines by experts in those disciplines. Recently, there has been both public and congressional concern about the *effectiveness* and fairness of the peer review system at the NSF. [Emphasis added.]

Analysis of the concept of "effectiveness," however, never appears in these studies. The chair summarizes the work as follows:

> What will you find in this study? First, it describes how the NSF decided which proposals were to be funded in 10 programs of their basic science research division for the fiscal year 1975. Second, it gives primary attention to the pivotal role of the program director in the decision-making process. Third, it contains a statistical analysis of the characteristics of applicants, such as age, institution, and citations to previous work. Fourth, it correlates those characteristics with the recipients of grants. In addition, it correlates peer review evaluation with the awarding of grants.
>
> To our minds, the major findings of the study are:
>
> 1. There is a high correlation between review ratings and grants made.
>
> 2. In the aggregate, there appears *not* to be a high correlation between grants awarded and the previous scientific achievement ("track record") of the applicant. (Author: This is an extraordinarily damning finding.)
>
> 3. Reviewers residing in major institutions were *not* inclined to treat proposals from scientists at major institutions more favorably.
>
> 4. Age had no strong effect on either ratings received or the probability of receiving a grant.

Notice the lack of overlap between these questions and the problems raised in the summary above, especially on one of the major concerns of science policy—distributions among fields. Notice that, in an era of an eroding U.S. technology base, there is no mention of the bias against applied science about which Edward Teller, Henrik Bode, Arthur Kantrowitz and others have written, and no mention of the BIG science versus little science balance, about which Harvey Brooks, for example, has written.

In spite of the fact that such major policy questions raised by the foremost U.S. science policy analysts are totally different from the questions addressed by agency-funded studies, the representatives of the science establishment (e.g., the presidents of the professional societies and the National Academy) invariably have used the publication of such reports to trumpet once again the vindication of "The Peer Review System." A typical *Chemical & Engineering News* report (7 March 1977) used the headline "NAS President Lauds NSF Peer Review System" in reporting Philip Handler's press release on the Cole, Rubin, and

Cole study. More naive scientists go much further, attributing the success of U.S. science, indeed, the success of science itself to peer review.[14] Thus is the paradigm reinforced.

Other Funding Systems for Science

The common reactions of many scientists that the present proposal review system (designated by the acronym PRP) is the only way to fund science, while tenaciously held, are, of course, absurd. Science flourishes all over the world without the use of individual proposal peer review. Indeed, while the foundations of modern physical science were laid from 1900–1950, PRP did not exist. Even within the United States, and with the exception of the health sciences, PRP is not used by the largest supporters of basic research—that is, by most of the U.S. Department of Defense, by NASA, by the Bureau of Mines, or by the Department of Energy. Many of the premier U.S. research funding agencies such as Office of Naval Research and Defense Advanced Research Projects Agency have never used PRP and continue to use a "responsible manager" system. Indeed, in the physical sciences and engineering, a substantial portion of the university research support comes from agencies *not* using the PRP. The entire apparatus of basic (and applied) research of U.S. industry manages very adequately without PRP and industry scientists continue to make substantial contributions to basic physics research in this country.

If PRP is but one system among many in use in the United States and elsewhere, then the type of research needed on PRP is quite obvious. (a) There is clear need for a study to compare the quality and quantity of research funded by different agencies that use different funding mechanisms. For example, one could easily compare the total solid state physics programs of the NSF, ONR, and Bell Labs or IBM with respect to papers and citations (and possibly society awards) per dollar of support. (b) There is also a need for study of how the typical research scientist funded by more than one system evaluates those systems with respect to such parameters as time-effectiveness, encouragement of innovation, or the time constants of the funding process. It is striking that this obvious piece of research has never been carried out by advocates of PRP, in spite of continuing requests for such a study.

In addition to the differences in the agenda for how to evaluate PRP, there are differences in how PRP systems are used by different agencies, (see Roy[15]). Indeed, different branches of the NSF may use substantially different systems. I have distinguished five levels:

(a) Mail reviewers selected by program managers (e.g., NSF Directorates of Mathematical and Physical Sciences) and letter "grades" averaged;
(b) mail reviewers selected by a committee of peers unknown to program managers (e.g., ARO(D));
(c) mail reviewers exist as in (a) but the reviews are discussed and evaluated by a different panel of peers (the process used in the NSF Biological Sciences Division, for example);
(d) mail reviews as in (a), discussed and rank ordered by a panel of peers, ratio of funds allocated to subfields by peer panel (NIH);
(e) mail reviews as in (a), panels as in (c) or (d) and site visit by panel (followed in the NSF Education Directorate, and some parts of NIH).

Obviously, these five systems are very different, and many criticisms of the mail review only system [system (a)] cannot be leveled at systems such as (d) and (e), which are typical of NIH.

Not only has the PRP process been subject to criticism, but, more importantly, there has been no good reason or theory for public funding of basic (atelestic) science at all. Only the economists C.F. Carter[16] and even more pointedly, H.G. Johnson[17] and most recently Milton Friedman[18] have raised the issue of *why* the public sector should fund such non-purpose-related basic science at all. Alvin Weinberg's[19] justification that basic research is the necessary "overhead" on all of national R&D is, I believe, the only response which could withstand scrutiny in the public domain. I base much of my new approach on combining the *how* of funding research to the *why*, following along similar lines.

A Proposal for an Alternative System

I have criticized the absence of a theory underlying the PRP system as it is practiced and so it behooves me to discuss the conceptual bases for the system I advocate. I propose some cardinal principles (based on the sheer experience of research managers worldwide) that underlie my alternative system:

(i) Past success is the best predictor of future performance. (The Bible states it thus: "You do not gather figs off thistles.")

(ii) Supporting small groups* (or individuals) on a continuing basis for the appropriate time constants (in, say, seven–year terms) increases the probabilities of success and efficiency of the system.

(iii) The most innovative science is done with the minimum of micro-management (i.e., checking on individual project goals or methods or of budget categories) but falls explicitly within the constraints of broad technical, social or national goals:

(iv) In most cutting-edge sciences, cooperation among the best specialists, and articulation of the state-of-the-art capabilities, has become the *sine qua non* for advance. Here the dismal failure of the universities to institutionalize interdisciplinary research[20] bodes ill for their role in the future where both interdisciplinary and inter-institutional cooperation will be demanded.

(v) Where huge sums of public sector support are involved, the best science must incorporate the public interest, however evaluated. Gerald Holton[21] phrases it thus: "A historic transition has occurred in the direction of basic research policy . . . for any professional activity has a just claim to moral authority when and only when it is widely seen to honor both truth and the public interest."

All the systems described below must reconcile two *apparently* opposite trends: greater local freedom for the investigator and greater accountability to the public. Accountability, *not* merely for honest expenditure of funds, however, but for the goals of research as well.

The Peer-Reviewed-Productivity Formula (PRPF) System

Let me now turn to a description of my alternative system for funding basic research. The "theory" supporting this approach starts with accountability to the public that pays the bill. The analysis begins by answering the economists' question: Why the public should fund such research with no specific missions or goals. What are the products of such research at a university? I identify four such types of products.

(a) New knowledge made available to the world at large via published research.

(b) Students trained at an advanced level (MS and Ph.D. principally) by apprenticeship in the research process, who become the staff of the entire *national* technical effort.

(c) Establishment of a research capability—in terms of people, new knowledge, research instrumentation, etc.—enabling the university to participate in the more telestic (i.e., with purpose, targeted, mission-oriented) research funded by mission agencies like DOE, DOT, OSHA, and DOD).

(d) Potential for a university to conduct research in support of (i.e., directly coupled to) the private industrial sector which provides a major part of the employment and economic base of the society which funds the research.

These are the major, quantifiable benefits society can expect from its atelestic research involvement.

In keeping with the view that the best predictor of success is the past track record, the rationale for this system then says that funding should be *proportional* to the past productivity (i.e., the benefits returned to society) of the research group or researcher in the aggregate of the four areas enumerated above. In practice, as in other institutional funding schemes, the government grants are *not* to individuals but to a group typically of the size of a department or interdisciplinary laboratory. (See below for schemes for funds allocation *within* the group and for arrangements for young investigators.)

The system would operate as follows: The agency division—e.g., in chemistry—would maintain records submitted annually by every institution of higher education conducting research and/or graduate education. The data supplied would include the following information on the "productivity" in each of the main categories enumerated above.

(a) The number of papers published mainly in an agreed-upon set of journals by the faculty in the unit (department or laboratory). Each faculty member can only be counted as one, although her/his work may be split over two or more units, with no double counting of product permitted. At a later

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[*] Lee DuBridge once said, "Whenever you find a highly successful group, I suggest you seek the cause of its success . . . by finding a man or small group of men who have created the spirit of the place and who knew how to preserve that spirit."

stage, citation indices may be used; but certainly for the first stage, as Roy, Roy and Johnson[22] have separately shown, there is little difference between papers and citations as productivity measures when one aggregates 20 to 40 faculty over three to five years.

(b) The numbers of M.S. and Ph.D. degrees granted, in each unit, each year. A simple weighting scheme of 3 × Ph.D.'s + 1 × M.S.'s will reflect fairly on the relative effort *if not the value of this "product."*

(c) The appropriate measure of how well the particular research capability serves national needs is the amount of research support obtained from mission agencies. This dollar amount is the aggregate research support from all mission agencies for the faculty in this unit, and it measures the unit's utility to the public sector.

(d) The final term is analogously measured rather easily by aggregating the total support for research (not gifts) received from industry and quantifies the unit's value to the private sector.

These data have been easily available for several years. The terms actually used would be a rolling average of the last three (or four) years. A great advantage of the system is that no step-function changes up or down are possible, preserving the continuity essential in long-term basic research.

The Combined Formula

The actual formula, based on the data provided, would provide for a weighting of each of the four terms as follows (where all data are rolling three-year averages):

Total $ to unit
= A × # of Publications
+ B × (# of M.S.'s + 3 × # of Ph.D.'s)
+ C × $ vol. of research support from mission agencies
+ D × $ vol. of research support from industry.

The weighting factors A, B, C, D, can be adjusted by the agency or by the Congress to fit the total budget. Obviously, these weighting factors would be adjusted to fit the characteristics of the field; where there is little or no mission agency or industrial support, the factors C and D could be zero. The *relative values of these terms* becomes a simple adjustable policymaking tool. It is easily understood by agency heads and Congresspersons. For example, if a member of Congress wants to encourage university-industry coupling, then no one needs elaborate schemes drawn up by bureaucrats far from the action, which cannot possibly be optimized for different disciplines, different industries, and different institutions. All Congress has to do is to raise sharply the weighting factor D in a particular field, and the signal of this incentive immediately goes out throughout the land. Likewise, if personnel shortages are acute *in one area* and there is a surplus in another, Congress simply rules that, for example, the factor B will be tripled in engineering and halved in theoretical chemistry or whatever.

An Objective, Productivity-Based, Peer-Reviewed System

This "formula" is, of course, radically different from the many other formula schemes proposed, in that it is both related directly to "productivity" in the areas for which funding is given and that it is not static. Many of the nation's foremost science policymakers in Congress had long ago seen the gross defects of the PRP system. Congressman George Miller of California, Chairman of the House Science and Technology Committee, Subcommittee on Research, introduced his famous "formula funding" bill in 1968 (his formula did not involve any *output* measures at all).[23] But the most radical aspect of the present formula is that it is totally based on performance. The present formula also involves the most intensive and defensible peer review in each area, and hence should immediately command the support of all those who believe in the peer evaluation by scientists:

(a) The vast majority of scientific publications are all in *peer-reviewed journals*. Hence, the worldwide peers have, via the process of peer review of *completed research*, passed on the quality of the work. This process of peer review of completed research is itself under attack, but it is vastly more secure than the PRP process. In most cases, counting *all* publications instead of just the peer-reviewed publications, would, in technical fields, make hardly any difference.

(b) The granting of advanced degree production is also almost always, except in some M.S.

degrees, protected by collective *peer* faculty judgment.

(c) The successful securing of research grants from strong managers operating in a mission agency is similarly conducted after a *review by technical peers* that is *much more stringent and meaningful* than any mail review of a proposal.

(d) Finally, by far the most stringent *peer review* is that which occurs when a research manager in an industrial laboratory is willing to back her/his judgment and set aside $50,000 to support a research proposal from a university.

Let us compare this PRPF formula system to the PRP system and to the defects we have alleged in the latter (see above):

(a) The PRPF provides a clear "theoretical" basis for the system. It is a proportional reward by society for delivering the "products" to society for which it is designed. Moreover, it is based on recorded, quantifiable performance not on the "promises" in an essay.

(b) The "best" science is defined as that which honors both scholarship and the public interest and which is "quantified" in the four terms of the formula.

(c) We have different sets of peers for different evaluations. In most of these, the group is *much* more broadly cognizant of other fields, of the national interest, or of industry's wants, than in the PRP system.

(d) Conflict of interest is eliminated. The PRP system is unavoidably riddled with conflicts of interest: a single negative review by one colleague, possibly competing for the very same funds, can stop an entire research project, with less than six months' notice. No single person can affect drastically the immediate future of any applicant. This elimination of conflict of interest goes much further than the commendable although mild effort by the National Endowment for the Humanities via its warning statement.

(e) Because the recipient is not constrained by any proposal whatsoever in the NSF, NIH, or elsewhere, she/he can follow any chance lead, any wild opening. It would be the most "basic" research funding ever devised. She can chose to do the most risky experiments in her atelestic work, by balancing it out with her mission agency and industry work. Alvin Weinberg's thoughtful proposal made 20 years ago that atelestic research should be an "overhead" on mission-oriented research will have been realized.

(f) The magnitude of an individual's research funding will prove to be slowly varying functions of time, guaranteeing every researcher a certain proportion of the truly stable funding to do *some* of the difficult work requiring five or more years, now shunned for quick payoffs in papers. The very fine feature of this formula is that it has no *sharp* cut-off in quality, of persons or universities. Thus it corresponds much more accurately to the realities of the distribution of talent.

This scheme is much more populist in that respect but *not* in an entitlement sense. It rewards productivity of the same "commodities," wherever produced. A wider range of universities will get *some* money on a steady basis, even if the total will not be large.

(g) It is not obvious that this funding scheme will by itself increase cooperation. It will, however, certainly diminish dishonesty, which as Rosalyn Yalow[24] has pointed out, may be as harmful as anything else to the science system. It will make cooperation a little more likely.

Commonly Raised Objections to the Proposed Systems

"The formula does not handle the beginning investigator"

The formula is designed explicitly to distribute research funds to the main research cadre, starting five to seven years after entry into the system. There are a very large number of alternatives for new investigators, including peer review of proposals, special young investigator awards based on performance and/or proposals. But the *formula* itself proposed herein can handle the new (young) faculty member in several ways. My considered preference is that any unit that adds new faculty members will receive one (or more) extra share(s) equal to the average research support for each faculty member in the unit. Of course, if the new person is a replacement, there is presumably already enough to support the replacement. This averaging scheme in my view is less perturbing to a local university structure than a fixed amount young investigator award.

"The formula delivers money to a department or similar unit and it will not go to the deserving"

One of the immediate concerns about this scheme is that it could reward the lazy members of the faculty. Obviously, such imbalance should be guarded against and it is simple to do so. Many universities already do exactly what is proposed here in the reallocation of the "Return of Overhead" to departments and research units. Each unit will be required by the funding agency to provide an "acceptable plan" so that individual research workers benefit from their own productivity. Typically such a plan might say that 5% to 10% of the funds will be retained at the college level for major equipment or projects for which any unit or individual can compete. Similarly 10% to 15% of the funds might be retained at the departmental or unit level for commonly used equipment and technicians. The 75% to 85% would then be divided up within the department, using the same (or slightly modified) formula so that the most "productive" faculty would receive the most of this genuinely unfettered support.

"The formula will be 'manipulated' by scheming scientists"

This manipulation is certain to occur—just as the same scientists naturally try to "manipulate" the present system. What does manipulation require? Writing more papers or turning out more advanced degrees. But surely these are "protected" by much stronger peer review gatekeeper processes than PRP. Hence, those who believe that the peer review process (of completed papers or theses) can be so easily manipulated, can hardly make a compelling case for continuing a much more tenuously justified process in the case of PRP. Any rush to publication is open to all and hopefully will be contained by toughened peer review standards in journals.

"Industry can flood the system with money in order to bias the direction of basic research"

Industry provides roughly 5% of the research funding of the major universities. If all the present schemes to increase this are wildly successful, this number may increase to 10%. This percentage varies from field to field. In some fields it may have no importance at all, in others, considerable. But the elegance of the formula is that the formula funding provided is *utterly unconstrained* with respect to subject matter, and hence industry would be helping truly unconstrained research in any obscure field if the investigator so chooses.

"Established laboratories would get most of the money"

One complaint that this system may be able to answer is that they do that *now* under the present PRP system. The "productive" groups in all aspects of science from theory to engineering would have equal access to the benefits without the biased hierarchical judgments of which is more basic science. In fact, one real merit of the system is that any (small) research unit can get *some* money, *and on a continuing basis*, and if "productive," can build that up systematically. At present such units are funded in a sporadic, wasteful pattern.

Conclusions

The gross inefficiency of the PRP system is being more widely acknowledged. The Cole, Cole, and Simon[25] summary showing the importance of chance in selection of reviewers and hence on the reviews of proposals, and the Ceci and Peters story[26] about the absurdities in peer reviewing *of papers* have each raised serious questions among the most fervent advocates. It would appear safe to predict that a "paradigm shift" is launched and mail peer review of proposals as now commonly practiced will disappear in a decade. This change will, I believe, be part of many likely similar changes in United States science policy and practice, as detailed in the book *Lost at the Frontier*.[27]

Acknowledgement—My ideas and critique of the peer review system have benefited enormously by long discussions with my colleagues who are program managers, and senior officers of the National Science Foundation who share the goal of trying to improve the system.

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DISCUSSION

Mr. PACKARD. Thank you, Dr. Roy.

You present a very interesting case. Apparently you have been propounding your case for some years and, as you, in your own statement, it has fallen on—not on deaf ears, but it certainly has not brought results. How do you account for that lack of change?

Dr. ROY. Oh, I'm not discouraged. It takes about 20 years to change anything, and this generation will die out. The very fact that language in your hearings, it's so reminiscent of what I—you know, it is now recognized. So I'm not discouraged. By the time I retire I think we'll have beaten them.

Mr. PACKARD. In your testimony, the—you have refuted the concept that science leads to applied science which, in turn, leads to technology and thus prosperity, or success. What does lead to technology and prosperity if it isn't through the channels that you've discounted?

Dr. ROY. Mr. Price, Derek de Solla Price at Yale, who was a great historian of science—he died a couple of years ago—he used the term "on-the-shelf science." Science is utilized by the technological system. Necessarily so. But I can get that science from any part of the world. What I need for technology is venture capital. I need trained labor. I need local infrastructure. I need a dozen different things to make a technology go. One of those things is new science. Now where do I look for that new science? I don't have to have it next door because that comes over the satellite at the speed of light. I can pick it up—we just stole from the Japanese how to make diamonds in our lab. There will be a big announcement this week. We learned how to make the diamonds. The Japanese stole it from the Soviets. They didn't steal it, but the science information went and I happened to have a student there, he showed me how to do it, and the Navy picked up the money, in 3 weeks; no peer review. The United States is further ahead because we are now making diamond films.

Now that's the way science works. That is now a technology in Japan—they've got a 3-year lead—which they picked up from the Soviets. It is a kind of machine for utilizing science.

Mr. PACKARD. It's true that some countries like Japan, being a good example you've alluded to, do a better job than many other countries, our own included, in the applied science area. However, Japan, I think, you're aware is now beginning to concentrate more and more of their resources on pure and basic science, recognizing that their primary weakness is not having the seed corn but simply using someone else's resources. And the United States, of course, has been one of the major resource areas for pure and basic science information throughout the world.

You can't develop applied technology if in fact there are not those resources of knowledge and pure science. Where would we get the pure science if in fact there were not countries like the United States and others that concentrate in that area almost as it were at the expense to applied science? If all countries simply did what Japan is doing, then there would be a famine of pure science research.

What kind of a balance should there be and should that balance be an international balance? Should it be controlled internationally by some organization, or should each country make their decision based upon what they feel is in the best interest of their own country?

Dr. ROY. I think the latter of course. I think in national science policy we must be concerned about the national good, the well-being. The last hearing I was at was with the Prime Minister of Japan, he invited five of us to come and advise him on how to organize their science and technology for the year 2000, so I was doing the same thing with the Prime Minister. His issue is very interesting. All of us, the foreign scientists—only five of us were asked—we all said do what you're doing well, forget all that other science jazz. Now that was a unanimous recommendation by all of us who were invited. He didn't like that, but nevertheless, that is what we said.

Let me come back to your question. It's a very interesting one. Let's take a football team that has a lot of good quarterbacks. We got nobody to catch the ball, so we're throwing balls all the time. The United States is full of quarterbacks with a good passing arm, and no receivers. So what's the use of getting more quarterbacks. So if I were the coach of Penn State—Joe Paterno, my friend—if I would say, "Hey, Joe, let's get some more quarterbacks; we're good at throwing balls, somebody will catch it out there." The Japanese are catching our balls. So what's the sense in us getting more quarterbacks?

You said the word balance. I believe we're unbalanced toward the size of receivers and we have too many quarterbacks. We have too much science production. That's heresy in my business. They'll shoot me outside, you see. We have too many basic science types running around and saying "I'll write papers." I've written 500 papers. Now what is a paper? I mean, what good does that do you? You're paying for it; $800,000 a year you pay me. To write papers? To increase my glory? What is that all about?

What am I doing for the country? I'm being paid by the citizenry of the United States. It's public funds. Why shouldn't basic science go to the private sector? Why shouldn't a supercollider be built by private sector? Mr. Hunt can build it. Why don't we do that? It's a pure consumption good. So I suggest that the balance is skewed.

Of course we need to have our own basic science generation. Nobody is going to shut that down. That is ludicrous. But we mustn't say, which side are we weak on? I suggest we're weak on the users of science, not on producers of science.

Mr. PACKARD. I'm intrigued by two comments, or two debates that apparently have been going on in the science world which you alluded to. One, of course, is the concept that science is finite and that there may be an end of the vein. How do you know when you reach the end of it? Gold or silver is a very, very tangible object and easily identified. But ideas and science concepts are certainly not tangible and something that you can specifically say "Well, we've reached the end. There are no more ideas out there that can be explored, no scientific concepts that we have not already discovered, and therefore we've reached the end of the vein." How do you know when you've reached the end of the vein?

Dr. Roy. Let's continue with the mining analogy. In the mining business there's never any such thing as no copper. There's always a little copper. But the richness of that ore, you've got to make a human judgment if it is worth it. Then, as you know, in many mines now we turn to the mill tailings—the tailings have more ore than some of the ores that we've been mining. So in that sense it never runs out, except it becomes not worth it.

Is it really worth it to spend all that energy to get out a little knowledge which has marginal benefit to the person who is paying it? There is no black and white in saying there is nothing to be learned. What Mr. Weisskopf was saying is the stuff that really connects to human life. If a person turns to the supercollider and says, "Oh, well, I could learn something about science, about the structure of the subnuclear world," and then he alleges it's the same as quantum mechanics, then he's lying. Mr. Penzias, a Bell Labs vice president, who is also a Nobel Prize-winning physicist, is the one that has pointed this out time and again. Quantum mechanics affects everything that we do, and whereas it is not true of the subnuclear world. So we can get knowledge, but is it worth it?

Mr. Packard. Well, assuming then that science, and I certainly don't necessarily agree to that concept, but let's make the assumption that science is a finite world and that we are coming near the end of the productive part of science—where it is no longer worth it. What then? Where do we go with our science world?

Dr. Roy. That's what I ask you to think about. That's for Mr. Fuqua and yourself to think about; that is to say, what are we doing with this committee?

If we address the question of policymaking with the atmosphere that we have an infinite pool, we do certain investment strategies. But if I say, "Wait a minute, it's getting more expensive, shouldn't I be switching over to more applied science?" And what do I mean by applied science? You see a problem of the human race is to learn to live with nature and themselves, and we never run out of those problems. But how to learn something more about the precise interrelationship between electrons and photons or something, that is a different game I think. There is a limit to how efficiently we can extract new information that we can use.

So maybe what will happen, there will be a drift toward applied sciences. We're seeing that I think worldwide.

Mr. Packard. It appears to me that there have been, during my lifetime and perhaps even during most of our parents' lifetime, the concept that we have advanced so quickly in our technology that there are no more places to go. I remember as a child reading an article that was extolling the tremendous advancement of mankind in being able to develop a machine that would take them 30 and even 50 miles an hour. And they were just developing the airplane then, too, they felt they had reached the epitome. There were no more, no more mountains to climb, and, obviously, we've climbed the Himalayas since then.

How can you know? How can you feel that there are no more mountains to climb when that same concept has been in existence ever since the Industrial Revolution began? It has been the buzz word almost of a certain segment of our society that we've discovered it all, how could you go any further than this? And since then, of course, have come the Space Age and the biomedical and biotechnology advancements that has literally transformed the human race.

Dr. Roy. You get me in all these unpopular positions and here I am saying but now it has become conventional wisdom since Lord Rutherford said there's nothing more to learn in physics that everyone says that proves that there is something more to learn in physics. That's a syllogism. That isn't correct. I believe that today we have excellent evidence. Prof. Stephen Kline at Stanford in mechanical engineering has written a nice set of papers on this. That everything is asymptoting. And let me give you an example from the one you used about the speed of travel.

The first Messerschmidt 262, or whatever it was, the first jet fighter, was running around in 1943, and we aren't going any faster in public transportation than that. We asymptoted.

Let me give you the nuclear weaponry. We went up to 10 megatons and then the Soviets built that big whopper, 70 megatons, we built a big whopper; then we said, we don't need those big ones. We came down.

The transmission of information is asymptoted at the speed of light. We can get it from Nairobi today on the evening news; it comes at the speed of light. You know what, Congressman? It's not going to go any faster. We're not going to get information any faster, any broader band than we have today. That's a terrible shock to most people. That the history of our lifetime, which you properly alluded to, was that funny part, that steep growth in experience and therefore we said, "Oh, my God, it's going to go on like this." No. I'm giving you examples that are physically limited, that are limited by economics, and last, but not least, by the fact that from Kennedy Airport to downtown is a fixed time, we'll never travel any faster. It doesn't make any sense to go any faster.

Mr. Packard. I don't dispute your concept that we have neglected the applied science area in this country. I think the chairman knows that that's been one of the questions I continually ask, is what are we doing wrong in this country that doesn't allow us to keep pace with other countries of the world in applying the knowledge that we gain, we glean through our scientific projects and research? And I think we can and ought to do a greater job.

Your novel idea of—maybe it's not a novel, maybe it's only novel to me because it's new—of granting, or providing grants for think time is a very interesting idea. I guess the only questions that I would have there would relate to how do you monitor that? How do you measure it? Is it measurable to know what kind of production you get, especially with budget constraints as we now know? How do you know that you're getting the best use out of those grant dollars if it's simply granted for people to think?

And I agree we don't think, we don't stop and meditate and sharpen the saw enough. We sometimes are so busy that we don't have the time to think. Sometimes I think legislators are that way. I'm convinced I would do a much better job if I had a portion of my time, a significant portion, a quarter of my time where I could sit and just think of how to implement ideas or develop ideas. I think we would be better legislators. And obviously, you feel the same about scientists.

But how do you monitor that? How do you measure it?

Dr. Roy. But do we monitor what we do with those instruments? I know a lot about them. I'm in the high-pressure business. I can now show you at least 80 big high-pressure presses provided by your generosity and the people of the United States which are lying rusting away because we overinstrumented. Most of these big instruments are money sinks. First, you've got to pay the service charges annually. Then you've got to have a technician. I get myself a $500,000 transmission microscope and it's costing me $100,000 a year to operate. Universities don't think about that. They think, "Oh, my God, I've got another gizmo."

Now how do we monitor the effectiveness of our instrumentation programs? Let me give you the computer program business. What is happening in schools today? Last week at the NSTA meetings you heard the diatribes against the computer introduction into schools. What do they do for us? We overinstrumented. Why does that happen? Because we've got a lobby out there called an instrumentation industry which—rightly in a free enterprise society—is pushing its case.

But who is arguing the case for thinking time? Now I would suggest that thinking time may be monitored by saying what did this guy produce in the way of papers when he taught? But from an instrument, you know, we turn the crank and in about 2 years 10 people use it well.

I'm not suggesting we abandon instruments. I'm suggesting we have a lower percentage of instruments and share them more effectively. Sharing is a very useful way. Have centers where people go. For a particular high-grade person, it's the human being that counts, not the instrument. At Cambridge, they still have that sealing wax and string tradition, and they manage to get a few Nobel Prizes here and there.

Mr. Packard. I'm taking more time than I should. And I'll ask one more question and then turn it over to the chairman for his questions.

If we scrap the peer review system and base the system on performance and on experience—I think that's what your recommendation is—would this or could this not lead then to an elitist group that receives all of the, all of the grants, and also could that not lead to abuse and perhaps cronyism and favoritism and subjection to lobbyists and to political pressures by Members of Congress or others without the checks and balances?

Dr. Roy. I think in the strong manager system you might say that is possible. But if you look at the record of the ONR, I would say it is as free of abuse, of cronyism and elitism as anything. Yes, it may be elitist in that sense they've choosed the best guys. But my formula thing is totally impervious to the ones you mentioned. You haven't had the chance to look at that, Mr. Packard, but, by and large, there's no way it can be manipulated any more than the present system, because it is so diverse. You can't change the number of Ph.D.'s. You can change the number of papers you write. Fine, go write some more papers. Furthermore, the most important is that everybody gets something. It is better distributive science than is the present system.

Let me give you one example I learned with that because today Mr. Keyworth said—last week he said we put out $10 million in NSF and we got proposals for $2.1 billion. Now he thinks that's a victory. I think that's a terrible loss because most of us set American science back because we were out there writing proposals which produces zero instead of doing our work. But five institutions got $2 million. Now that's elitism. That's stealing from the poor to give to the rich. Because these poor guys are being suckered into writing proposals which don't have a prayer. They're never going to make it, but they're ginned up by their deans and presidents to write proposals. Now, the guy should be writing papers, doing research. Instead, he's writing proposals.

Stimulation of proposal writing is a crime against science. I use that word advisedly. It is a crime against the scientific community and against the national interest. And if you permit that to happen and continue, I suggest you're not doing your job. If you say, hey, go out and put up a flag and sucker people into writing proposals.

Mr. PACKARD. Let me extend that question to one more group. And that is how does a new bright young scientist break into the field, or into the system then?

Dr. ROY. I have provided for that in my system in that paper. See, I don't fund individuals, I fund groups essentially—and a department or something. If they put in a new person they get 5 years—that person counting as a member of the team, and they're automatically provided for—they're given a chance to get a young person up to speed and make his contribution as a member of the team, as an apprentice. We think every young person should be producing, fighting against the others. That can't work. We have to have a seedbed, and my system provides for that seedbed.

Mr. PACKARD. I thank you very much, Dr. Roy, for your comments.

Dr. ROY. Thank you, Mr. Packard.

Mr. PACKARD. Mr. Chairman.

Mr. FUQUA. Thank you, Mr. Packard. And, Dr. Roy, I apologize for being late this morning. But I've had a chance to review your testimony and found it very enlightening and thought provoking.

Dr. ROY. That was the idea. [Laughter.]

Mr. FUQUA. I noticed that you disagreed with the Bush report that basic research leads to technology. Would you agree that that's true in all cases, or are there some cases where it does lead and in other cases that it does not?

Dr. ROY. Mr. Chairman, actually, as you know, I wrote a book about the Bush report, so I've read it very thoroughly. I think what happens is we took selective quotations from the book—we, I mean the science community—and alleged that that's what Mr. Bush said. Actually, Mr. Bush, as you know, called for a National Research Foundation in which everything was put together. He explicitly said that science should be in the service of humanity, of society, and that therefore the science, the basic science part, was a component for the national defense, the public welfare—that's the term he used, by which he meant industry—and so that if we had a system, and I mean an honest to God, like the Defense Department system, 6.1, 6.2, 6.3, 6.4, 6.5, then we would have all the basic science we want and it would be plugged into something that's using it. I think that basic science leads to technology when put in the context of a system, and that's the system that Bush envisaged. But when it is out here by itself it can't do that.

Mr. FUQUA. Well, are there areas of technology where technology innovation flourishes without being based on scientific advances?

Dr. ROY. Oh, my dear friends in Japan seem to get along very well. They haven't had a Nobel Prize for so long. They seem to use the science from all over the world. And not only ours. You're quite right that we produce a very large fraction of that science and the Japanese put it to use. I'm not even worried about the Japanese. I'm worried about the Chinese. I mean they're going to cut us off at the knees. I mean they have places growing crystals—you know, our infrared detectors which pick up the Soviet submarines are based on crystals which are grown in Czechoslovakia. I said, "My God, this world is coming to an end—that we use Czechoslovak technology based on our science." We taught them that at Penn State in 1966. They went home and create crystals. They give us the crystals which we give to the Defense Department which puts them into satellites. It is based on our science, but they have learned to grow better crystals than we have.

The Chinese are growing YAG crystals which will end up in Livermore. We can't grow them bigger than this. Now, that's technology. But if you say that is based on our science, well and good. But they have been willing to put in the manpower, the resources, to make science into technology.

Mr. FUQUA. Very good, very good point.

Mr. Packard asked one of the questions that I was very interested in and we have throughout the course of this study: How do you bring in the young or new investigator, into the system and make sure that the system does not block that person from being able to come in? And of course, you indicated that under that formula that that would be possible for the young investigator.

You also mentioned our former beloved chairman, George Miller, and I was one of the cosponsors of that bill, as a matter of fact.

Dr. ROY. I'm sorry. I had forgot that.

Mr. FUQUA. In Florida, it was known as the Fuqua-Miller bill.

Dr. ROY. Right. [Laughter.]

You can see how—that I'm not a politician. [Laughter.]

Mr. FUQUA. No, I was just being facetious. But I know that at that time we had gotten some very good results from block grants to universities. We still had the individual investigator awards. But we are subjected to the political system that unfortunately, is probably going to remain with us for a number of years—at least I hope so with all its faults—but where you have disruptions in fundings, up and downs. It would seem to me that block funding would be subject to more disruptive action in funding cutbacks and so forth than if you had individual smaller-type grants. I'm being somewhat of a devil's advocate in asking this question.

Dr. ROY. Well, I can see that you've thought it through because in a sense that was one of the potential critiques in the formula system that is a modified version of that Fuqua-Miller bill, which is really to say it's productivity linked.

Mr. FUQUA. Yes.

Dr. ROY. And if—it will go in a Gramm-Rudman type of thing. If there's a cutback, you can turn down some of the coefficients and you can turn down the total volume. But the amount that I get will be determined only by if I'm an active scientist, publishing papers, turning out Ph.D.'s, helping industry.

Mr. FUQUA. Um hum.

Dr. ROY. Now, if there's a Gramm-Rudman facing us, we'll cut back. Everybody will be cut back 10 percent. So in that respect, it will be less subject to fluctations. The worse thing I think in our present system is you go for a couple of years, you have grad students onboard, and all of a sudden something—and some peer review, one guy, can determine that. It's laughable. I mean, I talk to my colleagues in France, in England, they say, "You guys are mad." Who's a peer of Linus Pauling? Who is his peer? Some assistant professor of chemistry? I mean, what do we mean by peers?

I have suggested here, by the way, that a modification of the peer system which would correspond to our democratic society would be that the peers must be selected according to the national spectrum in that field. In materials science 80 percent of the scientists work in industry, but my peers are alleged to be only in universities, largely. I suggest that the simple legislative action which says the peers should represent just as the jury does. You know on the local jury, that has to be a lot selection. Why shouldn't we have that system?

Mr. FUQUA. Good point. Good point.

Well, you have indicated, peer review is riddled with conflicts with interest and is not broad enough; it's been alleged that it's kind of a continuation of an old, good old boy network of people, and so forth. It's kind of like the seniority system in Congress, it's not perfect but what do you replace it with? You have proposed an alternative.

Dr. ROY. Yes.

Mr. FUQUA. Has the peer review system brought about bad science? Have the results been less than they should have been?

Dr. ROY. I'd have to answer yes. Less than they could have been. Of course it hasn't brought about bad science with that much money. I do studies on the cost of Nobel Prizes, for instance. You know, it would be much cheaper for us to rent the British to turn out our Nobel Prizes. They're much cheaper. They're about one-tenth the cost, between a quarter and a tenth the cost of getting a Nobel Prize. If the objective of U.S. science is to win Nobel Prizes, rent the British; they're cheap.

But if we ask have we done bad science with our peer review? No. If you put that much money on anything, it's got to produce something. I give you the agriculture example here. In Florida, we have put too much fertilizer in so many places it's hurting. So that the wise application of fertilizer is to be desired, but overapplication is not necessarily good. I think peer review is an invaluable principle. We shouldn't be doing it by pork-barrel-type of things. Absolutely. But I believe there's many ways of doing it, and the present way his, as you mentioned, so many defects, especially efficiency. I really am here to make a plea for saying we are suffering in the trenches.

Writing these proposals, beating our brains out to produce an essay. Nobody ever checks whether I did that work, by the way. You know. If I write a proposal, I can say I'm going to invent them and the guy says yes, and I go and do something completely different and nobody checks it.

So what does the peer review mean? It's simply, as you say, it's a validation but 50 percent of it is luck and the other 50 percent is who you chose to review it.

And the manipulation that is possible. I don't think people are sitting around manipulating. All these good honest citizens in NSF and NIH, they're doing their best job. But is that system an efficient way of giving out the money? I really believe we should experiment with others. I don't think we should shut down peer review. I think we can optimize it. I think we can. But we must experiment with alternatives.

Mr. FUQUA. What would it do to large projects, and I'm not talking about big science, but, say, in high energy physics where you have an accelerator located there or you have other big ticket items and they keep coming back as high energy physics, or some form of physics, that, if under a formula, how would these programs fare under that? Because they get probably a disproportionate, if you're talking about the small investigators getting $15,000 to $50,000, or $75,000, which I think compared to the person that's getting $1½ million because of the nature of the project. How would it function on a comparable situation as that?

Dr. ROY. I think that at present what you call big science, high energy and radio astronomy, and so on, that really is in a protected pool. And I've suggested that the allocation among fields such as between chemistry and biochemistry and nuclear and particle physics be looked at. And you're the representatives of the people. It's a very tough judgment call. But within that package I believe that the same principles can apply. But I think big science is really a separate packet on which you have major policy decisions. My only recommendation there is that we internationalize that to the maximum extent possible. We really cannot afford a lot of consumption—good science. But how to select between particle physics and chemistry, that's what we pay you gentlemen for. It's a very tough call about what are we getting out of particle physics, what are we getting out of radio astronomy. It is my view that relatively we're underinvested, let's say, in chemistry—not my field—but in chemistry as compared to particle physics. But that's a judgment that is a very big and difficult judgment.

As you know, the British have moved very strongly in that direction against some of their big science efforts. I believe that that is the trend of the times. I can't see any other way. We're not going to build three supercolliders. And in some ways I think that's a separate call, as to how to distribute the money. That's a very much more difficult one.

Mr. FUQUA. You've summed it up. How do we distribute the money?

Dr. ROY. But that's within fields and between fields. As among fields, I think that is really at the highest levels of policy. Within fields, we've got many systems and I think that we need to experiment.

Mr. Fuqua. Thank you very much.

Dr. Roy. Thank you, Mr. Chairman.

Mr. Packard. I think that's one of the functions and purposes of this policy committee, is to try to determine just how those distribution of funds can best be directed toward a national policy that will be effective over the next decade. So it is a difficult question.

When the chairman commented about the seniority system, I thought there was some application with what we're discussing here. It was Sam Rayburn that said that the longer you—to a group of freshmen Congressmen—the longer you're here, the more you're going to like the seniority system. [Laughter.]

And I think that the universities that continually get the grants like the system that provides them with the grants. But be that as it may, it's obvious that you're a grain of sand in the science oyster, and I hope that you will continue to needle away. Because I think it's good for any industry as sensitive as the science industry to have those that will be constantly questioning the system that we seem to take for granted.

Thank you very much, Doctor.

Dr. Roy. May I make one comment on that last statement? I don't think I'm any longer in the minority of people who really strenuously are angered by the inefficiency. We're all winners. We're not talking about the people who come in and say "I'm not getting any money." I'm talking about the most successful American scientists getting money by the peer review system. We are now at the point of saying to you, "Look, this system is inefficient; it is chewing up our blood." That's a different thing from saying, "I'm not getting money." I believe that that has to be looked at.

I asked Carolyn for a list of the previous witnesses. It's a little biased toward the successful in the sense in industry. I'm terrified of what's happening to our manufacturing base. I consult with several companies. I'm in the industrial world. I can only use the word "terrified" as to what's happening to us in our technological base. You don't have enough people from steel, from glass, from magnetics. We see all these high technology characters, and semiconductors. A semiconductor is a trivial industry. It's small compared to magnetics. How many people have you had from the magnetics industry, which is a bigger industry than the semiconductor industry. And we're killing it. We're killing it. But those guys are not here because they're fighting for existence.

So maybe you should get a mix of those areas of our science and technology where we're clearly starting to lose out. It is cheaper to save a job than to create a new one by a factor of about 6 to 10, somewhere in that range. And we're not saving our jobs. We're giving them away and then trying to create new ones. I think we should look for some of the hurting people, not necessarily unsuccessful, but hurting people in the science and technology community.

Mr. Packard. Don't misunderstand. I recognize that pearls come from grains of sand that needle constantly and sometimes it takes a while to get there.

Thank you very much for your very stimulating testimony, Dr. Roy.

Dr. Roy. Thank you, Mr. Chairman, Mr. Packard.

Mr. PACKARD. At this time in the hearing, we're delighted to welcome Ruth Kirschstein to the microphone, who will be our next witness.

Dr. Kirschstein is the Director of the National Institute of General Medical Science, comes to us from Bethesda, and we're grateful for her presence.

Feel welcome to the committee. And we'll, again, your testimony and any other materials that you would like to have entered into the record by unanimous consent will be done so, and you may read, you may—you may summarize your statement, and I hope to say it in the words that the chairman often does, as long as it's not any longer than your written testimony.

I'll apologize. I may have to leave and turn the chairmanship to the Chair, or to the chairman. I have a short speech to give between 10:15 and 10:30 and I'll be back shortly thereafter, hopefully for questions, and I hope to hear most of your—so we'll turn the time to you, Dr. Kirschstein.

[A biographical sketch of Dr. Kirschstein follows:]

DR. RUTH L. KIRSCHSTEIN

Education.—A.B., magna cum laude, Long Island University, 1947. M.D., Tulane University School of Medicine, 1951.

Professional History.—1951-52, Internship, Medicine and Surgery, Kings County Hospital, Brooklyn, N.Y. 1952, Assistant Resident, Pathology, Veterans Administration Hospital, Chamblee, Ga. 1952-54, Resident, Pathology, Providence Hospital, Detroit, Mich. 1954-55, Trainee of National Heart Institute and Instructor in Pathology, Tulane University School of Medicine. 1956-57, Medical Officer, Resident in Pathology, Clinical Pathology, Clinical Center, NIH. 1957, Medical Speciality Board Certification, Pathology (anatomic and clinical pathology). 1957-60, Medical Officer, Pathologist, Laboratory of Viral Products, DBS, NIH. 1960-62, Chief, Section of Pathology, Laboratory of Viral Immunology, DBS, NIH. 1962-64, Assistant Chief, Laboratory of Viral Immunology and Chief, Section of Pathology, DBS, NIH. 1964-65, Acting Chief, Laboratory of Pathology, DBS, NIH. 1965-72, Chief, Laboratory of Pathology, DBS, NIH. 1971-72, Assistant Director, Division of Biologics Standards, NIH. 1972-73, Acting Deputy Director, Bureau of Biologics, FDA. 1973-74, Deputy Associate Commissioner for Science, FDA. 1974-present, Director, National Institute of General Medical Sciences, NIH.

Professional Organizations.—American Association of Immunologists; American Association of Pathologists; American Society for Microbiology; Institute of Medicine, National Academy of Sciences.

Committee Memberships.—Invited member of World Health Organization Expert Group on International Requirements for Biological Substances; Geneva, Switzerland, 1965 and 1971. Invited consultant to World Health Organization concerning problems related to the use of Poliovirus Vaccine, Live, Oral; Geneva, Switzerland, 1967. Member, Primate Research Centers Advisory Committee, Division of Research Resources, NIH, 1971-1975. Member, National Commission on Diabetes, 1975. Member, National Commission on Arthritis and Related Musculoskeletal Diseases, 1975. Chairperson, NIH Grants Peer Review Study Team, 1975-1976. Chairperson, PHS Genetics Coordinating Committee, 1976-1979. Member, Fogarty International Center Scholars-in-Residence Advisory Committee, 1977-1982. Member, NIH Coordinating Committee on Manpower, 1978-1983. Chairperson, PHS Task Force on Women's Health Issues, 1983-1984. Chairperson, PHS Coordinating Committee on Women's Health Issues, 1984-present. Member, NIH Advisory Committee for the IOM Study of the Organizational Structure of the NIH, 1983-1984. Member, HHS Executive Development Board, 1985-present.

Honors, Awards.—Elected to Institute of Medicine, National Academy of Sciences, 1982. Doctor of Science, *Honoris Causa,* The Mount Sinai School of Medicine, 1984. Honorary Doctor of Laws, Atlanta University, 1985. DHEW Superior Service Award, 1971. PHS Superior Service Award, 1978. Presidential Meritorious Executive Rank Award, 1980. PHS Equal Opportunity Achievement Award, 1983. PHS Special Recognition Award—group award to PHS Task Force on Women's Health Issues (Dr. Kirschstein, Chairperson), 1985. Distinguished Executive Service Award of the

Senior Executive Association, 1985. Presidential Distinguished Executive Rank Award, 1985.

STATEMENT OF DR. RUTH L. KIRSCHSTEIN, DIRECTOR, NATIONAL INSTITUTE OF GENERAL MEDICAL SCIENCES, BETHESDA, MD

Dr. KIRSCHSTEIN. Thank you, Dr. Packard.

As opposed to Dr. Roy, I am here today to talk about a different form of peer review and about issues that relate to biomedical research. The grants peer review system of the National Institutes of Health has been and still is the single most effective means of assuring that high quality health-related research is supported. I think it's fair to state that the peer review system developed by NIH has been emulated throughout the world and deserves much of the credit for the preeminent position of the United States in biomedical research today.

By way of background, peer review procedures for health research supported by the Federal Government were initiated back in 1902, and at that time the 57th Congress established a Scientific Advisory Board of nongovernmental scientists to assist the Surgeon General of the United States in the administration of the Hygienic Laboratory. In later years, that laboratory was renamed the National Institute of Health, singular. That became the National Institutes of Healths, and the advisory committee was reconstituted and renamed the National Advisory Health Council.

When the National Cancer Institute was established in 1937, the Cancer Act provided the legal basis for the National Advisory Cancer Council. That act specifically stated that the Council was to play a key role in recommending the award of grants. And these authorities and procedures have been extended to grants and fellowships in health research areas of all the NIH Institutes in 1944 when the Public Health Service Act was passed.

In 1946, it was recognized that there was a need to establish a more formal advisory mechanism for the assessment of technical and scientific merit by using a system of initial review groups—not single individuals, groups called Study Sections. And the Study Section review is the first level of the NIH peer review system. Each Study Section is composed of 14 to 20 nongovernmental scientists having national stature in a particular discipline—not in a narrow field, but in a broad discipline: biochemistry, hematology, surgery, genetics, molecular biology, for example. These initial reviewers, we believe, constitute the true scientific peers of the person submitting research grant applications.

The review groups meet three times a year for 2 to 3 days, and at the meeting each research grant application is presented and discussed individually in detail, after which there is a general discussion leading eventually to the development of a consensus among these peers. Each application is assessed for such factors as the objectives of the research, the research protocol, and the capabilities of the investigator based on his or her track record. Not only the number of publications, which many people feel is far too emphasized, but the quality of those publications, the quality of the scientific endeavors; the research environment in which the scientist works; the probability of success; the appropriateness of the budget to the task described; and the scientific significance of the proposed research.

The Study Section has three options and it's arrived at by majority vote: approval, if the application is deemed to have scientific merit; disapproval, if it's considered to be devoid of such merit; and if there is insufficient information, deferral for further information, asking of questions.

If a recommendation is made for approval, then each Study Section member individually, privately, having listened to the total discussion assigns a numerical priority rating based on his or her individual evaluation of the particular application. The possible scores range from 1, which represents the most favorable assessment, to 5, the least favorable. The assigned scores are then averaged and computed into a final overall priority score representing the entire group's opinions based on the long discussion.

These initial reviews are translated into a written summary statement that includes a critique, a recommendation, and, if favorably recommended, a priority score. This is the first level of the peer review system at NIH and here the system deviates from what is done, as nearly as I can tell, from most other organizations. The NIH peer review system is a dual one.

The second level occurs subsequent to the first and is carried out by the legislatively-mandated National Advisory Council, of which there is one for each Institute. Each Advisory Council has the broad function of advising on overall programs of the Institutes, on the balance between scientific disciplines, on areas that Dr. Roy mentioned, for example, as well as the legal responsibility for recommending those particular research projects for which funds are to be awarded. The National Advisory Council consists of 12 or more members, and one of its strengths is that some of these members are senior scientists and others are community leaders in areas relevant to the health areas and scientific responsibilities of the particular Institute. By law, an Institute may award a research grant only if it is recommended for approval by its Advisory Council, and if the Council does not recommend approval, an Institute may not award the research grant.

The Council's responsibility is to review the appropriateness of the assessment of the initial review and to make final recommendations, in addition, related to the program relevance and significance of the scientific endeavor and the priority of that research in relationship to the Institute's overall mission. By means of this dual system, there is a mandatory separation, therefore, of the assessment of projects purely regarding merit from the subsequent policy recommendations as to scientific areas of importance in which projects will be awarded and the level of resources to be allocated to those areas.

Now, as you know, over the years there have been a number of studies of NIH and studies of the peer review system. I have cited those in the references and I will not go through them any further. But these external committees, as well as the staff of NIH, have recognized the need for continuing surveillance of the system to assure that it functions well, remains responsive to the needs of the biomedical research community, and continues to fulfill its part of the NIH mission. Thus, in 1975, the then Director of NIH, Dr.

Donald Frederickson, established the NIH Grants Peer Review Study Team to conduct a detailed and comprehensive study of the NIH peer review system, and that is why I am here, to tell you about that.

The Study Team was asked to review the previous studies, analyze them, to articulate in detail the philosophy, objectives, procedures and accomplishments of peer review, and to prepare a document for dissemination both to the scientific community and the public. The members were appointed by the NIH Director and the Study Team consisted of NIH staff members. I was selected as the chairman. We recognized immediately that as an internal group we might lose credibility no matter what our recommendations for change would be because we could be considered self-serving, and therefore we set about to change this. We invited comments and suggestions from the external scientific community. We held three public hearings—Chicago, San Francisco and Washington. We had announcements soliciting various comments and critiques. And we did a detailed survey of the members of the initial review groups, the Study Sections and the Advisory Councils at that time. In short, we attempted to open up the system. We felt this was very successful. We received about 1,500 letters, the hearings were extremely well attended, and the return on the survey questionnaire was over 90 percent.

Several specific conclusions arose from this process, and I want to underscore that it was a process that took place about 10 years ago.

First, and most important, we learned that, at that time at least, the scientific, the biomedical research community didn't really understand the operation of the NIH peer review system even though it continued to be a beneficiary thereof. We felt that was a critical point and we were going to do something to correct it.

Second, the scientific biomedical research community endorsed the peer review system at NIH as the single most effective means of assuring the support of research of the highest quality.

Third, despite that endorsement researchers felt that although they might know more about their areas of research than anyone else there never was the opportunity to rebut any unfair scientific criticism presented during study section meetings.

And finally, the applicants felt that some sort of an appeals mechanism should be available to assure that each situation was considered justly and equitably.

Our study team listened very carefully to these criticisms. We reviewed the comments in depth. We studied many documents, and finally submitted a report to the Director of NIH in 1977. It was in three volumes and presented a detailed set of comments. One of the most important aspects of this report was the preparation of a thorough description of the peer review system and how it operated, so that any institution or individual could understand it. This was published in a series of three articles which were subsequently reprinted as a single document, and I have brought copies of these with me today.

In its report, the grants peer review study team made a series of recommendations by which the excellence of the system could not only be maintained but be improved, so that the continued high quality of biomedical research supported by NIH could be assured. The most important of these recommendations were: first, the announcement periodically of all upcoming vacancies on initial review groups, study sections, with an invitation for suggestions from the scientific and medical community regarding candidates for specific groups. This was a specific attempt to open up the system and get around what was perceived as a "old boy network."

Second, a formal procedure whereby an applicant investigator could: one, suggest the appropriate initial review group to which the application could be assigned; two, identify those particular aspects of the proposed research which he considered innovative and unique; and three, suggest possible consultants beyond the review group who were considered to be leaders in and currently knowledgeable about the area of research covered by the applicant.

Third, transmittal routinely to the investigator, and only to the investigator, not to his applicant institution, of a copy of that summary statement associated with his application.

And fourth, the establishment of a formalized NIH grants peer review appeal system. We suggested the creation of an ombudsman to whom investigators could appeal for advice.

This entire set of recommendations was published for review by the biomedical research community in an NIH publication in April of 1977, and the Director of NIH considered them and accepted the majority of them in another document titled "Decisions of the Director of NIH on Recommendations of the Grants Peer Review System" that he published in February of 1978, and I have a copy of that with me as well.

I'd like to look at each of the changes that have been instituted in some detail. NIH has periodically published a list of current advisory groups and their memberships in a book entitled "NIH Public Advisory Groups." This is distributed to all investigators and all institutions. Currently, however, above and beyond that, NIH now does announce vacancies on these groups and has developed a file of the names of individual scientists who wish to be considered for membership on such groups. Furthermore, nominations lists submitted by NIH officials for appointments to Study Sections must include appropriate representation of minority scientists and women.

Indeed today, in 1986, NIH often hears criticism that the scientists who are members of study sections are not "senior enough" and that the system has been opened up too far. But since these comments are about as voluminous as those saying that the system is still operating on an old boy network it seems likely that we have struck an appropriate balance.

Another recommendation that led to changes now has investigators being encouraged to and indeed making suggestions as to the particular Study Section to which a research grant application should be assigned, possible special reviewers to be used, and reviewers who should not be used because it is perceived that they have some sort of a bias. For the past 2 years every investigator has received a copy of the summary statement regarding his or her application not after the Council meeting, but before that; after the Study Section meeting and before the Advisory Council meeting. This provides an opportunity for a rebuttal to be submitted to the

Council. This system has worked very well. We have about 7 percent of the applications which are considered at any Council meeting having rebuttals associated with them.

And finally, about 2 years ago a formal appeal system, of which the submission of the rebuttal is the first stage, was instituted. This also seems to be working quite well but it seems to be a little bit too soon to really evaluate it.

I have been asked on many occasions why this extensive study has not been repeated, a study that was done 10 years ago. It was a study that took a great deal of time and a great deal of effort and we thought was very much worthwhile. But we feel that now with that basis we should provide a continued self-assessment addressing criticisms and corrections as needed. In fact, in the fall of 1984 the director of NIH held a full day's meeting of his own advisory committee on both the current peer review system and the mechanisms that NIH uses to support research. Out of that meeting came plans to establish two new types of research grants which would provide more flexibility to investigators and would answer some of the recent criticisms that we have heard.

One is an award to particularly meritorious, well-established investigators who have had NIH support for many years and who could be considered for further such long-term support, for example, up to 10 years, without having to resubmit a detailed proposal. This short statement of request for continued support would be submitted after the first 5 years.

The second award is to the new first-time investigator. Once such an award is made to a new scientist, one who has never had a research grant from NIH before, there is flexibility in the use of the funds for a period of up to 5 years, and this mechanism would recognize the fact that many new investigators need time to organize their laboratories, to obtain equipment, to hire staff to get going. Both of these new mechanisms are being implemented now, and I don't have any more information about them. We will be watching them very carefully.

I think it is essential to point out that since each of the review bodies, both the initial review level and the council level, indeed peer review, is made up of people, the peer review system has all the strengths as well as the weaknesses of humanity. The greatest strength of the system is the willingness and dedication of creative, highly skilled, productive basic and clinical research scientists who serve on the various panels and who spend countless hours in preparation, dedicate themselves to making the very best recommendations so that the funds available are used wisely. This requires time that they might otherwise devote to their own research, and much of that time is uncompensated. But I think it is fair to say that the people we have questioned feel that it is time very well spent and that they learn a great deal.

That the NIH grants peer review system is, despite any human frailties, one of excellence is attested to by the leadership of the U.S. biomedical research scientists in the world today. We have heard Dr. Roy state that the winning of Nobel Prizes is not a very important factor. I believe that in biomedical research it has been very important. And over the past 15 years, 20 of the Nobel Prizes in physiology or medicine and 12 of those in chemistry have been awarded to NIH grantees. Furthermore, young men and women come from all over the world to learn the skills of research in American biomedical research laboratories. But I would like to emphasize that the NIH research grants program is a competitive program. It is a program designed to provide support for the highest quality research. It is not a program designed to support research on a formula basis. And there is a corollary to that, namely, that some individuals will not make the grade.

On the other hand, we, at NIH, cannot afford to be complacent, and I don't think we are. We must continue to hone the peer review system, to listen to the critics, to reassess and make corrections as necessary. I also feel that the biomedical research community as a whole must reaffirm the value of peer review. And I believe that peer review is the foundation on which appropriate support of biomedical research depends and, as it has been said about liberty, the price of the support of biomedical research is eternal vigilance of the peer review system. I'll be pleased to answer any questions.

[The prepared statement of Dr. Kirschstein follows:]

NIH PEER REVIEW SYSTEM

RUTH L. KIRSCHSTEIN, M.D.
DIRECTOR, NATIONAL INSTITUTE OF GENERAL MEDICAL SCIENCES

I AM PLEASED TO BE HERE TODAY TO TELL YOU ABOUT THE GRANTS PEER REVIEW SYSTEM OF THE NATIONAL INSTITUTES OF HEALTH (NIH). THIS SYSTEM HAS BEEN, AND STILL IS, THE SINGLE MOST EFFECTIVE MEANS OF ASSURING THAT HIGH-QUALITY HEALTH-RELATED RESEARCH IS SUPPORTED AND THAT NEW CONCEPTS AND TECHNOLOGIES DEVELOP. I THINK IT IS FAIR TO STATE THAT THE PEER REVIEW SYSTEM DEVELOPED BY THE NIH HAS BEEN EMULATED THROUGHOUT THE WORLD AND DESERVES MUCH OF THE CREDIT FOR THE PREEMINENT POSITION OF THE UNITED STATES IN BIOMEDICAL RESEARCH TODAY. I WILL RETURN TO THIS THEME LATER.

BUT FIRST, SOME BACKGROUND:

PEER REVIEW PROCEDURES FOR HEALTH RESEARCH SUPPORTED BY THE FEDERAL GOVERNMENT WERE INITIATED IN 1902. AT THAT TIME, THE FIFTY-SEVENTH CONGRESS ESTABLISHED A SCIENTIFIC ADVISORY BOARD OF NON-GOVERNMENTAL SCIENTISTS TO ASSIST THE SURGEON GENERAL IN THE ADMINISTRATION OF THE HYGIENIC LABORATORY. IN LATER YEARS THAT LABORATORY WAS RENAMED THE "NATIONAL INSTITUTE OF HEALTH," AND THE ADVISORY COMMITTEE WAS RECONSTITUTED AND RENAMED THE "NATIONAL ADVISORY HEALTH COUNCIL."

WHEN THE NATIONAL CANCER INSTITUTE WAS ESTABLISHED IN 1937, THE CANCER ACT ALSO PROVIDED THE LEGAL BASIS FOR THE NATIONAL ADVISORY CANCER COUNCIL. THE ACT STATED THAT THE COUNCIL WAS TO PLAY A KEY ROLE IN RECOMMENDING THE AWARD OF GRANTS. THESE AUTHORITIES AND PROCEDURES WERE EXTENDED TO GRANTS AND

FELLOWSHIPS IN HEALTH RESEARCH AREAS OF ALL THE NIH INSTITUTES IN 1944, WHEN THE PUBLIC HEALTH SERVICE ACT WAS PASSED.

IN 1946 THE DIRECTOR OF THE NIH RECOGNIZED THE NEED TO ESTABLISH A MORE FORMAL ADVISORY MECHANISM FOR THE ASSESSMENT OF TECHNICAL AND SCIENTIFIC MERIT BY USING A SYSTEM OF INITIAL REVIEW GROUPS, ALSO CALLED "STUDY SECTIONS." STUDY SECTION REVIEW IS THE FIRST LEVEL OF THE NIH PEER REVIEW SYSTEM. EACH STUDY SECTION IS COMPOSED OF 14-20 NON-GOVERNMENTAL SCIENTISTS HAVING NATIONAL STATURE IN A PARTICULAR DISCIPLINE--FOR EXAMPLE, BIOCHEMISTRY, HEMATOLOGY, SURGERY, GENETICS, MOLECULAR BIOLOGY. THESE INITIAL REVIEWERS CONSTITUTE THE TRUE SCIENTIFIC PEERS OF THE PERSONS SUBMITTING RESEARCH GRANT APPLICATIONS.

THE REVIEW GROUPS USUALLY MEET THREE TIMES A YEAR, FOR 2-3 DAYS PER MEETING. AT THE MEETING, EACH RESEARCH GRANT APPLICATION IS PRESENTED AND DISCUSSED INDIVIDUALLY, IN DETAIL, BY PARTICULAR ASSIGNED REVIEWERS, AFTER WHICH THERE IS GENERAL DISCUSSION LEADING EVENTUALLY TO THE DEVELOPMENT OF A CONSENSUS. EACH APPLICATION IS ASSESSED FOR SUCH FACTORS AS: THE OBJECTIVES OF THE RESEARCH; THE RESEARCH PROTOCOL; THE CAPABILITIES OF THE INVESTIGATOR AND HIS OR HER ASSOCIATES; THE RESEARCH ENVIRONMENT; THE PROBABILITY OF SUCCESS; THE APPROPRIATENESS OF THE BUDGET TO THE TASKS DESCRIBED; AND THE SCIENTIFIC SIGNIFICANCE OF THE PROPOSED RESEARCH.

THE STUDY SECTION HAS THREE OPTIONS, ARRIVED AT BY MAJORITY VOTE: APPROVAL, IF THE APPLICATION IS DEEMED TO HAVE SCIENTIFIC MERIT; DISAPPROVAL, IF IT IS CONSIDERED TO BE DEVOID

OF SUCH MERIT, OR DEFERRAL. IF INSUFFICIENT INFORMATION IS PRESENTED FOR FORMULATION OF A FAIR AND RATIONAL RECOMMENDATION, THE STUDY SECTION MAY RECOMMEND DEFERRAL IN ORDER TO OBTAIN THE NEEDED INFORMATION OR TO RESOLVE ANY PROBLEMS. IF A RECOMMENDATION IS MADE FOR APPROVAL, THEN EACH STUDY SECTION MEMBER ASSIGNS PRIVATELY A NUMERICAL PRIORITY RATING BASED ON HIS/HER INDIVIDUAL EVALUATION OF THE PARTICULAR APPLICATION. THE POSSIBLE SCORES RANGE FROM 1 TO 5, WITH 1 REPRESENTING THE MOST FAVORABLE ASSESSMENT AND 5 THE LEAST FAVORABLE. THE ASSIGNED SCORES ARE THEN AVERAGED AND COMPUTED INTO A FINAL OVERALL "PRIORITY SCORE" REPRESENTING THE ENTIRE GROUP'S OPINIONS.

THE INITIAL REVIEWS PERFORMED ON EACH RESEARCH GRANT APPLICATION ARE TRANSLATED INTO A WRITTEN "SUMMARY STATEMENT." THIS INCLUDES A CRITIQUE, A RECOMMENDATION, AND, IF FAVORABLY RECOMMENDED, A PRIORITY SCORE.

AS I SAID, THIS IS THE FIRST LEVEL OF THE REVIEW SYSTEM. HOWEVER, IT MUST BE STRESSED THAT THE NIH PEER REVIEW SYSTEM IS A DUAL ONE. THE SECOND LEVEL OCCURS SUBSEQUENT TO THE FIRST AND IS CARRIED OUT BY THE LEGISLATIVELY MANDATED NATIONAL ADVISORY COUNCIL, OF WHICH THERE IS ONE FOR EACH INSTITUTE. EACH ADVISORY COUNCIL HAS THE BROAD FUNCTION OF ADVISING ON THE OVERALL PROGRAMS OF THE INSTITUTE AS WELL AS THE LEGAL RESPONSIBILITY FOR RECOMMENDING THOSE RESEARCH PROJECTS FOR WHICH FUNDS ARE TO BE AWARDED.

A NATIONAL ADVISORY COUNCIL CONSISTS OF 12 OR MORE MEMBERS, SOME OF WHOM ARE SENIOR SCIENTISTS AND OTHERS, COMMUNITY

LEADERS IN AREAS RELEVANT TO THE RESPONSIBILITIES OF THE PARTICULAR INSTITUTE. AN INSTITUTE MAY AWARD A RESEARCH GRANT ONLY IF IT IS RECOMMENDED FOR APPROVAL BY ITS ADVISORY COUNCIL. IF THE COUNCIL DOES NOT RECOMMEND APPROVAL, AN INSTITUTE MAY NOT AWARD THE RESEARCH GRANT. IT IS THE COUNCIL'S RESPONSIBILITY TO REVIEW THE APPROPRIATENESS OF THE ASSESSMENT BY THE STUDY SECTION AND TO MAKE FINAL RECOMMENDATIONS REGARDING PROGRAM RELEVANCE, SIGNIFICANCE, AND THE PRIORITY OF THE RESEARCH IN RELATION TO THE INSTITUTE'S MISSION.

BY MEANS OF THIS DUAL SYSTEM, THERE IS A MANDATORY SEPARATION OF THE ASSESSMENT OF PROJECTS PURELY REGARDING MERIT FROM THE SUBSEQUENT POLICY RECOMMENDATIONS AS TO THE SCIENTIFIC AREAS OF IMPORTANCE IN WHICH PROJECTS WILL BE AWARDED AND THE LEVEL OF RESOURCES TO BE ALLOCATED TO THOSE AREAS.

OVER THE YEARS, A NUMBER OF STUDIES OF NIH, INCLUDING STUDIES OF THE PEER REVIEW SYSTEM, HAVE BEEN MADE BY SEVERAL GROUPS EXTERNAL TO NIH. REPORTS SUCH AS THOSE OF THE WOOLDRIDGE COMMITTEE (1), THE RUINA COMMITTEE (2), THE WHITTAKER COMMISSION (3), AND THE FOUNTAIN COMMITTEE (4), HAVE EXAMINED ASPECTS OF NIH OPERATIONS AND ADMINISTRATION, HAVE DOCUMENTED THE STRENGTHS AND WEAKNESSES OF THE PEER REVIEW SYSTEM, AND, IN GENERAL, HAVE ENDORSED THE CONCEPT AND PHILOSOPHY OF PEER REVIEW AND RECOMMENDED CONTINUING THE USE OF THE SYSTEM AS THE EFFECTIVE MECHANISM BY MEANS OF WHICH RESEARCH GRANTS ARE SELECTED FOR SUPPORT. SINCE THESE REPORTS HAVE BEEN PUBLISHED AND ARE AVAILABLE, I WILL NOT DISCUSS THEIR CONTENTS FURTHER.

NEVERTHELESS, THESE EXTERNAL COMMITTEES AS WELL AS THE STAFF OF NIH HAVE RECOGNIZED THE NEED FOR CONTINUING SURVEILLANCE OF THE SYSTEM TO ASSURE THAT IT FUNCTIONS WELL, REMAINS RESPONSIVE TO THE NEEDS OF THE BIOMEDICAL RESEARCH COMMUNITY, AND CONTINUES TO FULFILL ITS PART OF THE NIH MISSION.

THUS, IN APRIL OF 1975 THE THEN DIRECTOR OF NIH ESTABLISHED THE NIH GRANTS PEER REVIEW STUDY TEAM TO CONDUCT A DETAILED AND COMPREHENSIVE STUDY OF THE NIH PEER REVIEW SYSTEM.

THE STUDY TEAM WAS ASKED TO:

- REVIEW PREVIOUS AND ONGOING STUDIES AND ANALYSES OF THE NIH AND OTHER SYSTEMS OF PEER REVIEW AND EVALUATE RECOMMENDATIONS MADE PREVIOUSLY.
- ARTICULATE IN DETAIL THE PHILOSOPHY, OBJECTIVES, PROCEDURES, AND ACCOMPLISHMENTS OF PEER REVIEW, AND TO PREPARE A DOCUMENT FOR DISSEMINATION BOTH TO THE SCIENTIFIC COMMUNITY AND THE PUBLIC SO AS TO ACHIEVE A GENERAL UNDERSTANDING AND APPRECIATION OF THE SYSTEM AS IT HAS EVOLVED TO ITS PRESENT STATE.
- EXAMINE IN CRITICAL DETAIL THE ENTIRE PROCESS OF PEER REVIEW AND MAKE RECOMMENDATIONS FOR MODIFICATIONS OR CHANGE. TO CONSIDER (1) SELECTION PROCEDURES AND CRITERIA FOR ADVISORY COMMITTEE MEMBERS, (2) THE NEED FOR AN NIH POLICY DEFINING THE ADMINISTRATIVE AND SCIENTIFIC RELATIONSHIPS OF THE STAFFS RESPONSIBLE FOR THE TECHNICAL MERIT REVIEW AND THOSE RESPONSIBLE FOR SCIENTIFIC PROGRAM MANAGEMENT, (3) THE NEED, ADVANTAGES, AND DISADVANTAGES FOR A TECHNICAL MERIT REVIEW "APPEAL"

MECHANISM FOR APPLICANTS. AND AN ASSESSMENT OF THE PROCESS CURRENTLY IN USE. AND (4) MOST IMPORTANTLY. THE CAPABILITY OF THE PEER REVIEW SYSTEM TO ACCOMMODATE A REALLY NEW OR UNUSUAL SCIENTIFIC IDEA.

THE MEMBERS WERE APPOINTED BY THE NIH DIRECTOR; THE STUDY TEAM CONSISTED OF NIH STAFF MEMBERS. AND I WAS SELECTED AS CHAIRMAN. WE RECOGNIZED IMMEDIATELY THAT. AS AN INTERNAL GROUP. WE MIGHT LOSE CREDIBILITY NO MATTER WHAT RECOMMENDATIONS FOR CHANGE WE MIGHT MAKE ON THE BASIS THAT WE COULD BE CONSIDERED SELF-SERVING. ACCORDINGLY. WHEN THE STUDY TEAM CONSIDERED HOW TO DEAL WITH THIS ISSUE. IT CAME TO THE CONCLUSION THAT BY INVITING COMMENTS AND SUGGESTIONS FROM THE EXTERNAL SCIENTIFIC COMMUNITY. WE WOULD OBTAIN INPUT WHICH COULD EXPAND AND EXTEND OUR OWN PERCEPTION OF THE SYSTEM.

THROUGH THREE PUBLIC HEARINGS. HELD IN CHICAGO. SAN FRANCISCO. AND WASHINGTON. THROUGH ANNOUNCEMENTS SOLICITING COMMENTS AND CRITIQUES OF THE PEER REVIEW SYSTEM. AND BY MEANS OF A DETAILED SURVEY OF THE THEN MEMBERS OF THE INITIAL REVIEW GROUPS AND ADVISORY COUNCILS. WE ATTEMPTED TO "OPEN UP" OUR STUDY.

THIS. WE FELT. WAS VERY SUCCESSFUL. WE RECEIVED ABOUT 1.500 LETTERS. THE HEARINGS WERE EXTREMELY WELL ATTENDED. THE RETURN ON THE SURVEY QUESTIONNAIRE WAS ABOVE 90 PERCENT.

SEVERAL SPECIFIC CONCLUSIONS AROSE FROM THIS PROCESS. WHICH I WANT TO UNDERSCORE TOOK PLACE IN 1975 AND 1976;

FIRST AND MOST IMPORTANT. WE LEARNED THAT. AT THAT TIME. AT LEAST. THE SCIENTIFIC COMMUNITY DID NOT REALLY UNDERSTAND THE

OPERATION OF THE PEER REVIEW SYSTEM EVEN WHILE OBVIOUSLY SOMEWHERE ALONG THE LINE IT CONTINUES TO BE THE BENEFICIARY THEREOF.

SECOND. IN GENERAL. THE SYSTEM WAS ENDORSED AS THE SINGLE MOST EFFECTIVE MEANS OF ASSURING THE SUPPORT OF RESEARCH OF THE HIGHEST QUALITY.

THIRD. DESPITE THE ENDORSEMENT. RESEARCHERS FELT THAT. ALTHOUGH THEY MIGHT KNOW MORE ABOUT THEIR AREA OF RESEARCH THAN SOMEONE ELSE. THERE NEVER WAS THE OPPORTUNITY TO REBUT ANY "UNFAIR" SCIENTIFIC CRITICISM PRESENTED DURING STUDY SECTION MEETINGS.

FOURTH. APPLICANTS FELT THAT AN APPEALS MECHANISM SHOULD BE AVAILABLE TO ASSURE THAT EACH SITUATION IS CONSIDERED JUSTLY AND EQUITABLY.

THE STUDY TEAM LISTENED VERY CAREFULLY TO THE COMMENTS AND CRITICISMS. AND ALSO TO THE PRAISE OF THE PEER REVIEW SYSTEM. IT REVIEWED THE COMMENTS VERY CAREFULLY. STUDIED MANY DOCUMENTS ON PEER REVIEW. DISCUSSED THE ISSUES THOROUGHLY AND. IN JANUARY OF 1977. SUBMITTED A REPORT TO THE DIRECTOR. NIH (5). THE REPORT WAS IN THREE VOLUMES AND PRESENTED A DETAILED SET OF DOCUMENTS. ONE OF THE MOST IMPORTANT ASPECTS OF THIS REPORT WAS A THOROUGH DESCRIPTION OF HOW THE PEER REVIEW SYSTEM OPERATES SO THAT ANY INSTITUTION OR INDIVIDUAL CAN UNDERSTAND THE METHODS USED. THIS DESCRIPTION WAS PUBLISHED IN A SERIES OF THREE ARTICLES WHICH WERE SUBSEQUENTLY REPRINTED AS A SINGLE DOCUMENT. A COPY OF WHICH I HAVE WITH ME TODAY (6).

IN ITS REPORT THE GRANTS PEER REVIEW STUDY TEAM MADE A SERIES OF RECOMMENDATIONS BY WHICH THE EXCELLENCE OF THE SYSTEM COULD BE NOT ONLY MAINTAINED, BUT ALSO IMPROVED SO THAT THE CONTINUED HIGH QUALITY OF BIOMEDICAL RESEARCH SUPPORTED BY NIH COULD BE ASSURED. THE MOST IMPORTANT OF THESE RECOMMENDATIONS WERE:

1) THE ANNOUNCEMENT PERIODICALLY OF ALL UPCOMING VACANCIES ON INITIAL REVIEW GROUPS (STUDY SECTIONS) WITH AN INVITATION FOR SUGGESTIONS FROM THE SCIENTIFIC AND MEDICAL COMMUNITY REGARDING CANDIDATES FOR SPECIFIC GROUPS.
2) A FORMAL PROCEDURE WHEREBY AN APPLICANT-INVESTIGATOR MAY (A) SUGGEST THE APPROPRIATE INITIAL REVIEW GROUP TO WHICH THE APPLICATION SHOULD BE ASSIGNED; (B) IDENTIFY THOSE PARTICULAR ASPECTS OF THE PROPOSED RESEARCH WHICH MAY BE CONSIDERED UNIQUE; AND (C) SUGGEST POSSIBLE CONSULTANTS (BEYOND MEMBERS OF THE REVIEW GROUPS) WHO ARE CONSIDERED TO BE LEADERS IN AND CURRENTLY KNOWLEDGEABLE ABOUT THE AREA OF RESEARCH COVERED BY THE APPLICATION.
3) TRANSMITTAL ROUTINELY TO THE INVESTIGATOR, AND ONLY TO THE INVESTIGATOR, OF A COPY OF THE SUMMARY STATEMENT ASSOCIATED WITH THE APPLICATION AS SOON AS POSSIBLE AFTER THE COMPLETION OF THE REVIEW BY THE NATIONAL ADVISORY COUNCIL OR BOARD.
4) THE ESTABLISHMENT OF A FORMALIZED NIH GRANTS PEER REVIEW APPEALS SYSTEM CENTRAL TO WHICH IS THE CREATION

OF AN OMBUDSMAN (A "FRIEND IN COURT") TO WHOM INVESTIGATORS CAN APPEAL FOR ADVICE, TO LEARN WHAT STEPS SHOULD BE TAKEN NEXT OR TO REBUT WHAT IS CONSIDERED AN "UNFAIR" ASSIGNMENT TO A REVIEW BODY OR AN "UNFAIR" REVIEW.

THE ENTIRE SET OF RECOMMENDATIONS WAS PUBLISHED FOR REVIEW BY THE BIOMEDICAL RESEARCH COMMUNITY IN AN NIH PUBLICATION IN APRIL 1977 (7). THE DIRECTOR, NIH, CONSIDERED THE RECOMMENDATIONS AND ACCEPTED THE MAJORITY OF THEM, INCLUDING ALL THE FOUR THAT I MENTIONED ABOVE.

LET US LOOK AT EACH OF THE CHANGES THAT WERE INSTITUTED IN SOME DETAIL:

1) NIH HAS PERIODICALLY PUBLISHED A LIST OF CURRENT ADVISORY GROUPS AND THEIR MEMBERSHIPS. CURRENTLY IT DOES ANNOUNCE VACANCIES ON THESE GROUPS AND HAS DEVELOPED A FILE OF THE NAMES OF INDIVIDUAL SCIENTISTS WHO WISH TO BE CONSIDERED FOR MEMBERSHIP ON SUCH GROUPS. NOMINATION LISTS SUBMITTED BY NIH OFFICIALS FOR APPOINTMENTS TO STUDY SECTIONS MUST INCLUDE APPROPRIATE REPRESENTATION OF MINORITY SCIENTISTS AND WOMEN.

INDEED, TODAY, IN 1986, NIH OFTEN HEARS CRITICISM THAT THE SCIENTISTS WHO ARE MEMBERS OF STUDY SECTIONS ARE NOT SENIOR ENOUGH AND THAT THE SYSTEM HAS BEEN "OPENED UP TOO FAR." SINCE THE COMMENTS IN THAT REGARD ARE ABOUT AS VOLUMINOUS AS THOSE SAYING THAT THE SYSTEM IS STILL OPERATED BY "AN OLD BOY NETWORK," IT SEEMS LIKELY THAT NIH HAS STRUCK A BALANCE.

2) INVESTIGATORS HAVE BEEN ENCOURAGED TO AND, INDEED, DO MAKE SUGGESTIONS AS TO (A) THE PARTICULAR STUDY SECTION TO WHICH A RESEARCH APPLICATION SHOULD BE ASSIGNED, (B) POSSIBLE SPECIAL REVIEWERS TO BE USED, AND (C) REVIEWERS WHO SHOULD NOT BE USED BECAUSE IT IS PERCEIVED THAT BIAS WOULD OCCUR.

3) NOW, FOR THE PAST 2 YEARS, EVERY INVESTIGATOR HAS RECEIVED A COPY OF THE SUMMARY STATEMENT REGARDING HIS OR HER APPLICATION AFTER THE STUDY SECTION MEETING AND BEFORE THE ADVISORY COUNCIL MEETING, PROVIDING AN OPPORTUNITY FOR A REBUTTAL TO BE SUBMITTED TO THE COUNCIL. THIS SYSTEM HAS WORKED VERY WELL; ABOUT 7 PERCENT OF THE APPLICATIONS BEING CONSIDERED AT ANY COUNCIL MEETING HAVE REBUTTALS.

4) A FORMAL APPEALS SYSTEM, OF WHICH THE SUBMISSION OF A REBUTTAL IS THE FIRST STAGE, WAS INSTITUTED ABOUT 2 YEARS AGO, AND THIS SEEMS TO BE WORKING QUITE WELL, ALTHOUGH IT IS TOO EARLY TO REALLY DO AN EVALUATION AS YET.

SINCE 1976-77, NO LARGE NIH STUDY OF PEER REVIEW HAS TAKEN PLACE. RATHER, CONTINUED SELF-ASSESSMENT, ADDRESSING OF CRITICISMS, AND CORRECTIONS AS NEEDED HAVE OCCURRED. IN FACT, IN THE FALL OF 1984, THE DIRECTOR OF NIH HELD A FULL DAY'S SESSION OF HIS ADVISORY COMMITTEE ON BOTH THE CURRENT PEER REVIEW SYSTEM AND THE MECHANISMS THAT NIH USES TO SUPPORT RESEARCH. OUT OF THAT MEETING CAME PLANS TO ESTABLISH TWO NEW

TYPES OF RESEARCH GRANTS WHICH WOULD PROVIDE MORE FLEXIBILITY TO INVESTIGATORS. ONE IS AN AWARD TO PARTICULARLY MERITORIOUS, WELL-ESTABLISHED INVESTIGATORS WHO HAVE HAD NIH SUPPORT FOR MANY YEARS AND WHO COULD BE CONSIDERED FOR FURTHER LONG-TERM (UP TO 10 YEARS) SUPPORT WITHOUT HAVING TO RESUBMIT A DETAILED PROPOSAL TO BE REVIEWED AFTER THE FIRST 5 YEARS. THE SECOND AWARD IS FOR THE NEW FIRST-TIME INVESTIGATOR. ONCE AN AWARD IS MADE TO SUCH A NEW SCIENTIST, THERE IS FLEXIBILITY IN THE USE OF THE FUNDS OVER A PERIOD OF UP TO 5 YEARS. THIS MECHANISM WOULD RECOGNIZE THE FACT THAT MANY NEW INVESTIGATORS NEED TIME TO ORGANIZE A LABORATORY, OBTAIN EQUIPMENT, HIRE STAFF, ETC.

BOTH OF THESE NEW MECHANISMS WILL BE IMPLEMENTED WITHIN THE NEXT FEW MONTHS AND, THUS, I HAVE NO FURTHER INFORMATION ABOUT HOW USEFUL OR SUCCESSFUL THEY WILL BE.

I THINK IT IS ESSENTIAL TO POINT OUT THAT SINCE EACH OF THE REVIEW BODIES, BOTH AT THE INITIAL REVIEW LEVEL AND THE COUNCIL LEVEL, IS MADE UP OF PEOPLE, IT IS CLEAR THAT THE PEER REVIEW SYSTEM HAS ALL THE STRENGTHS AS WELL AS THE WEAKNESSES OF HUMANITY. THE GREATEST STRENGTH OF THE SYSTEM IS THE WILLINGNESS AND DEDICATION OF CREATIVE, HIGHLY SKILLED, PRODUCTIVE BASIC AND CLINICAL SCIENTISTS WHO SERVE ON THE VARIOUS PANELS, WHO SPEND COUNTLESS HOURS IN PREPARATION, AND WHO DEDICATE THEMSELVES TO MAKING THE VERY BEST RECOMMENDATIONS SO THAT THE FUNDS AVAILABLE ARE USED WISELY. THIS REQUIRES TIME WHICH OTHERWISE WOULD BE DEVOTED TO THEIR OWN RESEARCH, AND MUCH OF THAT TIME IS UNCOMPENSATED.

THAT THE NIH GRANTS PEER REVIEW SYSTEM IS, DESPITE ANY HUMAN FRAILTIES, ONE OF EXCELLENCE IS ATTESTED TO, AS I INDICATED, BY THE LEADERSHIP OF THE UNITED STATES BIOMEDICAL RESEARCH SCIENTISTS IN THE WORLD TODAY. IN THE PAST 15 YEARS, 20 OF THE NOBEL PRIZES IN PHYSIOLOGY OR MEDICINE AND 12 OF THOSE IN CHEMISTRY HAVE BEEN AWARDED TO NIH GRANTEES. FURTHERMORE, YOUNG MEN AND WOMEN COME FROM ALL OVER THE WORLD TO LEARN THE SKILLS OF RESEARCH IN AMERICAN LABORATORIES.

I WOULD ALSO LIKE TO EMPHASIZE THAT THE NIH RESEARCH GRANTS PROGRAM IS A COMPETITIVE PROGRAM: IT IS A PROGRAM DESIGNED TO PROVIDE SUPPORT FOR THE HIGHEST QUALITY RESEARCH. IT IS NOT A PROGRAM DESIGNED TO SUPPORT RESEARCH ON A FORMULA BASIS. BUT THERE IS A COROLLARY TO THIS; NAMELY, THAT SOME INDIVIDUALS WILL NOT MAKE THE GRADE.

ON THE OTHER HAND, WE, AT NIH, CANNOT AFFORD TO BE COMPLACENT. WE MUST CONTINUE TO HONE THE PEER REVIEW SYSTEM, LISTEN TO THE CRITICS, REASSESS AND MAKE CORRECTIONS IF NECESSARY. FURTHERMORE, CONTINUED REAFFIRMATION OF THE VALUE OF THE PEER REVIEW SYSTEM MUST COME FROM THE BIOMEDICAL RESEARCH COMMUNITY AS A WHOLE.

PEER REVIEW IS THE FOUNDATION ON WHICH APPROPRIATE SUPPORT OF BIOMEDICAL RESEARCH DEPENDS. AS IT HAS BEEN SAID ABOUT LIBERTY, THE PRICE OF SUPPORT OF BIOMEDICAL RESEARCH IS ETERNAL VIGILANCE OF THE PEER REVIEW SYSTEM!

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DISCUSSION

Mr. **Fuqua**. Thank you. Thank you very much.

You noted on page 11 of your prepared comments about two new types of research grants and the associated methods of selection that have been instituted there. Do you see a further diversification of the methods of project selection, and are they desirable or undesirable? How has that worked in the practical application?

Dr. **Kirschstein**. I think, Mr. Chairman, that diversification for diversification's sake would not be appropriate. I think that as we continue to reassess what we are doing at NIH and as we listen to the scientific community we will make the appropriate corrections. If new mechanisms are needed, we will provide for them. But I don't think we should plan to do that unless there is a need to do so.

One must say that every new research grant mechanism that is developed will add to, unless one reassesses and take some away, the confusion that might be out in the community as to the best way in which to apply for research grants. I think we need to tell our story better, and we are trying to do that.

Mr. **Fuqua**. I guess much of the discussion about project selection has been on how you best review for scientific merit. To what extent does that bring in other factors such as the cost, the relevance to technology development or institutional needs, or the geographical distribution of funds? And I mean not that there be a formula that each State, but that at least it's not concentrated in one specific area of the country, or two areas or three or whatever. Are these considered, or should they be considered, these factors?

Dr. **Kirschstein**. I think the very strength of the NIH system is its two-tiered level of peer review. The factors that you have mentioned, such as distribution of funds, the relevance of the research to the needs of the biomedical research community as well as the needs of the country in regard to health care, are all areas that are considered by the advisory councils. Indeed, in the legislation providing for the advisory councils, those are the specific tasks that are assigned to the councils.

In regard to geographic distribution, there is no specific way at the moment, nor do I think there should be, to specifically give out research grants based on geographic distribution. However, we are required to assure that peer reviewers both at the level of the advisory council and the initial review groups are chosen from across the entire spectrum of States of the country.

Furthermore, there is one other new program that NIH has instituted based on the recommendation and the funds provided by the Appropriations Committee, and that is a program of grants to small college scientists. This has been going for about a year or so now and we think is working quite well.

Mr. **Fuqua**. Let me commend you for that. I think that's very important. I was not implying that the distribution of the grants should be based solely on geographic distribution, but a lot of other things being equal, to make sure that there was a distribution.

One of the recent concerns that has been expressed about the NIH peer review process is that increasingly some of the best scientists are declining to serve on the peer review committees because of the excessive amount of demands on their time. Is this a serious problem, or has it been exaggerated?

Dr. **Kirschstein**. I think it's been exaggerated. I think there was a period of time when right after the recommendation to open up the system was implemented that NIH began to appoint some younger scientists to the initial review groups and there may have been for a short while a preponderance of those. We are now beginning to balance that out very well. Just recently three or four members of the National Academy of Sciences have been reappointed because many of them have served in the past on the initial review groups.

Furthermore, the senior scientists are the individuals that we most often use on the Advisory Councils, and they provide a stability and a knowledge that they have gained over the years in the advice that the Councils provide to the Institutes and, indeed, during the Council meetings the Government officials, called executive secretaries of the initial review groups, sit in on the Council meetings and hear what goes on, so they take that back to their initial review group members as well.

Mr. **Packard**. Mr. Brown.

Mr. **Brown**. Dr. Kirschstein, do I pronounce your name correctly?

Dr. **Kirschstein**. Yes, you do, sir. Thank you.

Mr. **Brown**. I assume the various study panels maintain a statistical record of the number of qualified research applications versus the number of unqualified or deferred, and that you make comparisons between the study panels, and so forth. What is the proximate level of applications that are being funded currently?

Dr. **Kirschstein**. About one-third of the applications that are approved have been awarded over the last several years. In years past that was closer to 50 percent. It is a function, of course, of the funds that are available.

Mr. **Brown**. Yes. There is no particular magic in terms of the beneficial impact on research or any other criteria that you can think of for any particular percentage? In other words, you don't have any way of evaluating whether if you fund a third versus a half you get better results overall or anything of that sort, do you?

Dr. **Kirschstein**. I think we do. There is an enormous amount to learn in biomedical research. Dr. Roy told Mr. Packard that science was finite. And I cannot speak for the areas that Dr. Roy is involved in, but biomedical research has a long, long way to go. There is a great deal to learn. In fact, each solution to a particular problem that a scientist is studying leads to 5 or 6 more problems that open up and that are going to have an enormous impact on the health of this country and the world. If we cannot fund sufficient numbers of excellent research projects building on the creativity of the biomedical research scientists that we have in this country, then I think we will slow down the pace of what we can do to improve the health of the Nation. And indeed, we think an award rate of about 50 percent, of about half, is appropriate.

Mr. **Brown**. What happens to the other half? They don't stop the research do they?

Dr. **Kirschstein**. Some do. Some receive support from some of the private foundations. It's not very extensive. Some become affiliated with one of their more fortunate neighbors at universities and continue to try to do research. Some can occasionally be supported by the universities themselves. But I think we are losing an opportunity to obtain important information if we cannot continue to support creative scientists.

Furthermore, I do think that it is going to be a very, very, very long time before we really learn even a tenth of what we need to know in the arena of biological sciences. The message that is being sent to the young people of this country, if we cannot support good research today, is going to cause us to have a very small number of creative scientists in the years to come because people will not go into that field.

Mr. **Brown**. You mean, you're implying that scientists are motivated by the monetary reward?

Dr. **Kirschstein**. No. I'm implying that they need to make a living, and if they cannot receive a stimulus that assures them that they are going to be able to do the research they want to do, they may go into other careers.

Mr. **Brown**. How do we go about adapting our grant procedures to the changing opportunities in science? And let me just clarify that a little bit. We are continually making breakthroughs and determining fascinating new problem areas in science. It may be as a result of something like some tragic circumstance like the AIDS virus, for example, which opens up a new area that apparently needs to be researched.

Generally speaking, good scientists like to go where there are challenging problems. How do we go about adapting the grant process to this continually changing excitement or opportunity that exists from the nature of the science itself? Do you have the feedback coming from the scientific community and the administrative flexibility that allows you to make these kinds of shifts?

Dr. **Kirschstein**. Yes, I think we do. Because first of all, as you say so eloquently, the good scientists want to go where the opportunities are, and so they submit the applications in those areas.

Second, we have an ability to prioritize within the limits of the budget in regard to certain areas. The studies on AIDS virus, the breakthroughs that have been possible on AIDS virus would not have been possible without the basic molecular biology, virology and immunology that has been supported for the past 20 years. The ability to make an AIDS vaccine, which is being worked on now, is because of the development of recombinant DNA technology by U.S. scientists a bare 13 years ago. And what has happened since that time has been absolutely astonishing. We will be able to develop such an AIDS vaccine because of those initial discoveries supported by basic undifferentiated research.

Mr. **Brown**. Is there basic undifferentiated research being funded in each of the study panels across the spectrum, or is there someplace where you go particularly to get funding for basic undifferentiated research?

Dr. **Kirschstein**. Well, let me, if I may, say a little bit about the NIH system. The study panels are based on scientific disciplines. Biochemistry, that's a basic science. Surgery, that's an applied science. The funding is provided by the various Institutes, of which there are 12 or 13. Of the 13, 12 are Institutes that have a particular clinical- or population-targeted research mission. The National Heart, Lung, and Blood Institute, the National Institute of Child Health and Human Development. All those 12 support basic research but it's basic research that's related to those particular missions.

My own Institute, the National Institute of General Medical Sciences, does not have such a mission orientation. We are the basic undifferentiated, fundamentally-based research Institute at NIH, and so the research in molecular biology that now has allowed us to know about the AIDS virus was funded in this Institute. The work on the AIDS virus itself is now being done by the National Institute of Allergy and Infectious Diseases.

Mr. BROWN. Well, I doubt if I'll ever completely understand all the complexities of the process.

Dr. KIRSCHSTEIN. I'm sorry.

Mr. BROWN. Let me ask something that generally Congressmen have a little easier time understanding, and that is responding to population needs. For example, 15 or so years ago we decided to fund a major program in kidney dialysis based upon research breakthroughs that indicated that that could be done, but there's a great deal more that needs to be done in that area. More recently one of our committee members sponsored legislation to increase the funding for spinal cord injury research. Obviously there are constituencies behind these kinds of things.

Dr. KIRSCHSTEIN. Yes, sir.

Mr. BROWN. The question from the congressional standpoint is how do you go about meeting the needs of constituency groups? For example, the legislation with regard to spinal cord injury was a simple effort to get the Institutes to spend twice as much money on spinal cord injury research. The initial response was we know how much ought to be spent and it's exactly right and we don't want you tinkering with it, which is the common response that we get when we attempt to tinker.

Is there any basis for your saying that the amount that a particular Institute is spending on a particular kind of research is the magic number, rather than that which the Congressman responding to a constituency feels is the magic number? And I'm not saying that either one is necessarily right.

Dr. KIRSCHSTEIN. I agree with you. I'm not sure either one is necessarily right, and I do think what is appropriate is the balance between the two. I don't think we're all-knowing at NIH. I think we know pretty well at a point in time what the needs are. I think we need to keep ourselves always informed. We should have scientific meetings on the subject, but we also need to hear the constituencies. That, I think, is the great value of the Advisory Council having on it individuals who are not scientists but who are informed members of the lay public who have a particular interest in a particular field, and therefore bring that to bear on the decisionmaking that leads to how the Institutes make awards. That is very valuable, and I think the input that we get from the Congress in regard to need to spend in certain areas is enormously valuable.

Mr. BROWN. You just don't want to have to pay too much attention to it.

Dr. KIRSCHSTEIN. No; I think we should pay attention to it. I don't know if I'm talking for my colleagues, but I, personally, do think we should pay attention to it.

Mr. BROWN. Well, that's a perennial problem.

Dr. KIRSCHSTEIN. Yes, I know.

Mr. BROWN. And we have it not only in the field of biological science and basic research but in making the decisions as to the allocation of research funds between any field of science.

Mr. BROWN. And frequently within the field where there are different kinds of policy and opportunities. And I think it's fairly fundamental that we try and understand the criteria and the basis on which these kinds of decisions are made and not try from a congressional standpoint and do that which is unreasonable. But on the other hand, not necessarily accept everything that the scientific community says because they are biased in their own way.

Dr. KIRSCHSTEIN. I agree.

Mr. BROWN. I have no further questions, Mr. Packard.

Mr. PACKARD. Thank you, Mr. Brown.

I'm sorry I was not here to hear all of your testimony. However, I read it carefully last night and have jotted down some questions.

How are your representatives—the 14 to 20 nongovernmental scientists, and also in the second tier of your peer review system there's 12 or more—ultimately appointed?

Dr. KIRSCHSTEIN. The recommendations regarding individuals to serve on the initial review groups are made from many sources, from biomedical scientists throughout the country, from the members of the panel themselves who are going off who make suggestions, from the knowledgeable staff at NIH, as well as people who wish to serve and nominate themselves and are considered. All of those recommendations are put in some sort of perspective by the Government scientists and final nomination slates are sent forward to the Director of NIH, taking into consideration what the various Institute staff feel are important. Those people on the initial review groups are appointed by the Director of the National Institutes of Health.

The members of the Advisory Councils, by law, are appointed by the Secretary of the Department of Health and Human Services. Each Institute—and the Councils are for the Institutes and the Study Sections are for all of NIH—nominates individuals for the Council, as does the Director of NIH, and these are sent forward. But the final decisions are made by the Secretary and that depends very much on what the Secretary wishes to do.

Mr. PACKARD. I have to assume that those appointments are highly sought after.

Dr. KIRSCHSTEIN. Many of them are.

Mr. PACKARD. And is it highly competitive from one university to another?

Dr. KIRSCHSTEIN. The membership on the initial review groups is not necessarily highly competitive. It is an honor but it is also very hard work, and some people feel that they do not have the time. We were discussing that with Mr. Fuqua. The membership on the Advisory Councils is sought after by many people and it depends on the Secretary's viewpoints.

Mr. **Packard**. How vulnerable are they to political pressures once they're appointed and once they're doing their review and making recommendations?

Dr. **Kirschstein**. Once they are appointed they are not vulnerable to political pressures.

Mr. **Packard**. I'm talking now about in the selection process of projects and grants, not the selection of the person.

Dr. **Kirschstein**. No, I think they are not. First of all, they work as a group and it is clear that if any one individual were to try to unduly influence the system the other people on the Council or on the initial review group would not let that happen.

Second, because the funds that are available are very often not clearly known until quite late in the fiscal year, the recommendations regarding selection of particular applications by the Council are just that, they are recommendations. The final decision for the award of such grants is in the hands of an Institute Director such as myself, because there never are enough funds to support all of the recommendations of the Advisory Councils.

Mr. **Packard**. The system that we now use in project selection does involve a great deal of competitiveness from the different applicants. One of the comments, as you heard Dr. Roy make, was that an inordinate amount of time is used up by highly skilled scientists in developing applications. I'd like your response to that, but also I'd like to hear how you feel the competitiveness influences the demands for higher degrees of funding certainty. Well, let me restate that.

There is also a claim or an outcry that we're hearing for a better stable funding mechanism, so that we can develop our programs and not be constantly worried about our funding sources. There is a little bit of a conflict in those two areas, a cry for more funding stability as well as for the competitiveness, and so forth. How do you equate that? And would you respond to that, please?

Dr. **Kirschstein**. I think there is a little bit of difficulty between the two areas, but I think it can and has been resolved. First of all, by providing research grants not for 2 to 3 years as Dr. Roy mentioned and as is more customary in some of the other agencies, but for 3 to 5 years, and now with one of the new mechanisms up to 10 years, NIH is providing a stability of sorts. The scientists who do receive awards are assured of funds for 3 to 5, and in some cases 10 years.

Second, no scientist really wants to simply repeat a sure bet that his predecessor has undertaken. All scientists want to do something fundamentally important, and I think that leads to the desire to put forward innovative research. We have monitored this very carefully. I read all the summary statements that go to each of the Council meetings of my particular Institute. I've been doing that for 12 years. That's an awful lot of summary statements. And the number of times that the critique within them says this is really a new piece, an innovative, creative piece of work are really quite remarkable. Although there is this hue and cry that it is not so, I don't believe it is; not every piece of science that every investigator does is going to be the most original in the world. That's impossible. But there is a great deal of original work being done.

Within the limits of the funding that are available, the only way to make the best decisions as to what to fund at least partially has to be driven by competition. There are other things that drive it as well: the need for that area of research, the relevance of that research to the health problems of the country; and so forth. We've talked about all of them. But competition has to be a piece of that.

Mr. **Brown**. Would the gentleman yield?

Mr. **Packard**. This question of funding noninnovative research or only funding innovative research leaves me questioning how we get the research necessary to verify this brilliant research because it's fundamental that it has to be replicated, and it requires excellence in terms of scientific criticism which can only be based upon efforts to replicate that and to determine if it's valid. How do we get that kind of work done?

You, I presume, don't fund it as innovative research.

Dr. **Kirschstein**. No, we do fund it as verification—that's why I said that not every single piece of research that we support is "innovative." A great deal of it is building on that fundamental leap. In the course of building on it and expanding on it very often the scientist will repeat the work and verify it.

If an individual publishes a paper which is questioned, there are plenty of scientists out there who will go at it to prove or disprove whether it is true. I totally agree with you verification is very essential.

Mr. **Packard**. Materials research and biomedical research are often different in their scope and in their nature. Do they require in your judgment a different selection process? Does one peer review work better for one than it would another? Dr. Roy felt so.

Dr. **Kirschstein**. I can't really answer that question because I have never been involved in the area of applied materials research. I would, personally, believe that the basic tenets of peer review could be applied to both. That there may need to be some revision of peer review to more indicate in the materials science area what the specific application is going to be within a finite period of time may be true. In biomedical research, particularly basic biomedical research, we very often don't know whether the application is going to be when one learns about the cell to heart disease or cancer or neurologic disease or spinal cord injury. Indeed, it may be to all of them, but at the time of the start we may not know that. It may be that there is in materials science some differences, but I'm not really an expert of that.

Mr. **Packard**. Is there value in the high number of applications that are prepared, or is that wasted time and talent for scientists?

Dr. **Kirschstein**. I think in the preparation of applications the scientists do have to stop and take the time that Dr. Roy was talking about to think because it is not automatic to just simply write an application. I think one has to think about what one wants to do, why it's important to do it, and what you're going to gain. So I think there is thinking time and therefore there is value in doing it.

What I do not think is valuable is for a scientist who has done really quite well, in fact, very well, in the peer review system but because the funds are not available to award the grant to put in another application in hopes that application will be a little bit better and therefore he or she will be able to get the award. I think that that time is not well spent.

Mr. PACKARD. Thank you. It has been observed that while presently used NIH systems of project selection may work very well during times of rapid budget increases, during a budget crunch it doesn't work as well. What are your perceptions in this area?

Dr. KIRSCHSTEIN. Any system that is human is subject to that kind of distortion, and I think that is partially true. We work very hard, and must continue to do so, to prevent that kind of bias from coming through. But that is the area that I talked about where a scientist who has what we call a priority score right at the edge of whether there are sufficient funds to make an award, if he has to rewrite that application to get it 10 points lower I think that's too bad.

Mr. PACKARD. In many of our hearings it's become obvious that applications are, and projects are used to promote specific personal goals of some of the scientists—that they can market and use some of the products that come out of their research. And that's not bad, of course, but it does open up the door for abuse, for conflicts of interest, and so forth. How have you addressed that, and what are some of the problems that we struggle with in conflict-of-interest situations?

Dr. KIRSCHSTEIN. We try very hard to keep conflict-of-interest issues to a bare minimum. An individual must not participate in the review of any application from his own institution or, if it's an industrial firm, from his own firm. In fact, we expand that, as you probably know, to be from his own academic institution. For example, in the State of California all of the parts of the University of California system are considered to be his own academic institution.

Furthermore, the members of the staff of the Institutes and of the review panels, by reading the literature and knowing who are the people who are in serious competition with each other, try to keep individuals from participating in reviews who might have a bias because—I don't want him to do that work because I want to do it myself. Within the human system, I think we've done as well as we can with that.

Mr. PACKARD. In conclusion, do you think the existing system needs to be reformed or revised? Do you see a possible combination of other systems joined with the peer review system, or are you satisfied that the selection system now is adequately working to enhance the kind of research that we're looking for?

Dr. KIRSCHSTEIN. I am basically satisfied that the peer review system is working but I am never satisfied that we cannot improve it somewhat. I think we need to continually do that, and that is why I said in my closing phrase of my statement that we need to have eternal vigilance. I have dedicated my career at NIH to doing that.

Mr. PACKARD. One of the comments that Dr. Roy made, and it's in his testimony, was that perhaps added to, or an adjunct to the existing system a product-based, or a productivity-based formula system might be worthwhile to consider. Has NIH done any research or experienced any review of that possible recommendation?

Dr. KIRSCHSTEIN. The productivity of a scientist is part of the review of his or her particular project. The problem with consideration of productivity is the qualitative aspects. There are many scientists in the biomedical research area—I will confine myself to that area—who can publish a 100, 200 papers in 1, or 2, or 3 years. Some are in highly respected journals, some are in refereed journals, and some are in journals that are not refereed. By simply counting papers, I don't think we are really determining whether that has been work that is worthwhile.

A qualitative aspect added to the quantitative aspects of productivity is very important. And I think in the review that our study sections provide that is given.

Mr. PACKARD. In fact, are we underemphasizing the applied science area in this country, or do you feel that the balance between basic research and pure research is pretty well on target?

Dr. KIRSCHSTEIN. In biomedical research, I think the balance is about right.

Mr. PACKARD. In the materials research you're not sure?

Dr. KIRSCHSTEIN. I can't really speak to that at all.

Mr. PACKARD. I'd be happy to yield. I'm through, Mr. Brown.

Mr. BROWN. I want to pursue the point you raised just a little bit further. We customarily think in terms of funding research in the sciences that we have two systems: the project grant system which is exemplified by what you've been describing; and the formula grant system which is probably better represented by the Department of Agriculture. In one you fund an institution and the other you fund individuals, through an institution, of course, but seeking to get the best proposed research.

There is this third method which you can't completely distinguished—none of these are completely distinct, but I would say a third method would be the identifying of the most productive scientists, as he mentioned, and funding their research and taking potluck as to what the results would be. Presumably, if you were wise in selecting the best scientists to support and they were truly the most productive, you would have a constant stream of innovative discoveries coming out without the need to go through constant process of writing grant applications and the paraphernalia, reviewing them, and so on. You wouldn't completely avoid that because the process of determining who the most productive scientist is, is just as hard as determining what the best project applications are. But it conceivably could be an alternative. And I rather gather that you have seen the possibilities of this with your new investigator grants and your senior investigator grants which, in effect, sidestep the detailed review of specific grant applications. And what it seems to me is that fundamentally we're exploring combinations and alternatives keeping in mind that the final determinant is how much good science we get out of them.

What I'm concerned about is whether or not we are conducting this process in such a fashion as to be able to make reasonable judgments as to which of these systems or what combination of these systems is the best—best serves the Nation and the scientific world in the particular circumstances.

Now you have already commented on this, but could you just to comment again as whether you think the process of achieving the proper balance or combination is one which is receiving the thorough scrutiny which it needs and if there's some objective way in which we can measure whether that proper combination is being put into place?

Dr. KIRSCHSTEIN. I think the process is receiving scrutiny. I'm not sure it's receiving as much scrutiny as it probably needs, and that's one of the reasons why I said that I think we need to continue to assess the system constantly, to listen to the criticisms that come forward very carefully, and make changes as we go along. I think it should probably be done on a rally continuing basis. I know that Dr. Wyngaarden, Director of NIH, is concerned about this and is thinking about it repeatedly, and as indicated by having had his meeting of his Director's advisory committee and by discussions we have all had, and I think it must continue to be done.

Mr. BROWN. Well, I would hope that we could look to the scientific community and the scientific leadership to demonstrate the ability to do that, so that those of us in more mundane occupations could benefit from it. Because we need that same process in the Congress, you know. Institutional change is very difficult.

Dr. KIRSCHSTEIN. Yes.

Mr. BROWN. And getting a system for analysis and review of the need for institutional change is really the important thing.

Dr. KIRSCHSTEIN. I agree. This is not the only forum that I have been asked to address. Last November, I talked to the Association of American Medical Colleges on peer review—and because of my having been chairman of that study team I make a number of speeches about this—and I always make the point that we need to continue to reassess the system and that the biomedical research community has a very specific role to play in this and needs to exercise it continuously.

Mr. BROWN. Well, I'm hinting at the possibility that we are concerned here with the broader problem of how we ensure optimum social change in general, and if you've got any good speeches on that subject I'd be happy to hear them.

Mr. PACKARD. We want to thank you Dr. Kirschstein, for your excellent statement and for your answers to our questions. We're grateful that you've appeared before this committee.

And with that we will adjourn the committee hearing.

[Whereupon, at 11:10 a.m., the task force was adjourned, to reconvene at 9 a.m., Wednesday, April 9, 1986.]

RESEARCH PROJECT SELECTION

WEDNESDAY, APRIL 9, 1986

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to call, at 9:06 a.m., in room 2318, Rayburn House Office Building, Hon. Ron Packard (acting chairman of the task force) presiding.

Mr. **Packard**. Good morning, ladies and gentlemen. We would like to call this hearing to order. We sincerely appreciate the witnesses being here this morning.

Today is the second in a series of three days of hearings on "Research Project Selection." Yesterday we heard Dr. Rustum Roy discuss problems with our current methods for selecting research projects. Dr. Ruth Kirschstein discussed the peer review system as currently practiced in the extramural grant program of NIH.

Today we will explore how research project selection is accomplished in our Federal agencies. We will hear from Dr. James Kane, former Deputy Director of the Office of Energy Research of the Department of Energy; Dr. Marvin Moss, Director of the Office of Naval Research; Dr. Mary Clutter, Special Assistant to the Director of NSF; and Dr. Robert Berliner, former Deputy Director of Science at NIH. They will speak to us about in-house project selection at their institutions.

We sincerely appreciate each of you being here. We will hear from you individually and ask questions, but we would appreciate it if you would remain at the table in the event that members of the committee would like to ask cross questions as the need would require, if your time will permit.

So, with that, we would like to first hear from Dr. James S. Kane, Special Assistant to the President of the University of California, and former Deputy Director, Office of Energy Research in DOE. I appreciate, Dr. Kane, your being here and we now look forward to your testimony. You may summarize, you may read as you have submitted. All of your testimonies, along with any additional material you would like to have added to the record, will be done. So we'll proceed with you, Dr. Kane.

[A biographical sketch of Dr. Kane follows:]

Dr. James S. Kane

Dr. Kane received the B.S. degree in 1950 from Montana State University and the Ph.D. degree in 1955 from the University of California, Berkeley. He was at the Lawrence Livermore National Laboratory from 1955 to 1974, where he served as

Laboratory Scientist, Head of the Ceramics Division, and Head of the Chemistry and Materials Science Department. In 1974, he became Technical Assistant to the General Manager of the Atomic Energy Commission. From 1975 to 1977, he was Deputy Assistant Administrator for Conservation and later for Physical Research at the Energy Research and Development Administration (ERDA), the predecessor agency of DOE. From 1977 to 1981, Dr. Kane was the Associate Director for Basic Energy Sciences of the Office of Energy Research, and since 1981, he served as Deputy Director of the Office of Energy Research, Department of Energy.

STATEMENT OF DR. JAMES S. KANE, SPECIAL ASSISTANT TO THE PRESIDENT, UNIVERSITY OF CALIFORNIA, BERKELEY, CA; AND FORMER DEPUTY DIRECTOR, OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY

Dr. KANE. Thank you very much, Mr. Chairman.

I am going to confine most of my remarks just to a very brief summary of what I said in my written remarks. I will not read them or repeat them.

Mr. PACKARD. May I suggest that we move the microphone and turn it on so that we can all hear and also for the record?

Dr. KANE. To repeat myself, I will not read my testimony. I will hit a few of the more important points in it.

I would like to start off by commending the committee for these exhaustive and, I'm sure, sometimes exhausting review of the science policy in the United States. I think the science enterprise of the United States is running very well, but I also think it's time for its 40-year checkup.

I read with great interest the agenda that you sent me before I was asked to testify. I would like to depart from what I'm going to spend most of my time and just to highlight five of the points that were in that agenda that I think you and the science community in general will spend an awful lot of time on in the coming years. This is no particular order of priority.

The first one I'll mention is this new and kind of explicit mission. You know, after 40 years of science, there's really always been two missions—one was to do science for the sake of science, and the other was to do science for some particular application that that mission agency had as its responsibility, whether it be the National Institutes of Health, the Defense Department, or the Energy Department.

In the last few years I've seen a new mission creeping in, and it's not creeping in; it's there now, and that is research that will somehow make the country more competitive economically. How do we do this? It's a different mission. And so I think it's going to take a lot of your attention on how we can figure the research enterprise of the United States to take into account this new and very important goal. Because the answer may not be science as we've always done it in the past. We may have to do it differently because the goals of this science are to transfer it and transfer it, if possible, exclusively to the industry of the United States. That's my first point.

The second has to do with today's hearings. The process has gotten terribly complex in the last 40 years. There are really three processes involved in research. There's the proposal aspect of it; there is the contract granting aspect of it; and then there's the auditing after the contract aspect of it. And the last 40 years has seen what the Government-University-Industry Roundtable calls bureaucratic accretion in all of these processes. It's much more paperwork involved with all three aspects—the writing the proposal, the evaluation of the proposal, the first third; the middle third is the granting of the contracts—and the boilerplate on some of these is formidable—and finally, the auditing, the postresearch auditing, to make sure the dollars all went in the right places. It has become a very bureaucratic business. I think that deserves your attention.

The third point is the need for university facilities. I will not go into that. I'm sure it's been mentioned a lot, but it's something that deserves your careful attention.

The fourth point is the need for large scientific facilities, unique, dedicated facilities, to do work you cannot do any other way.

The fifth and most difficult of all—and you wouldn't get any advice out of me if you asked on this one—is a need for explicit prioritization of our science. I think we're very good within a discipline in prioritizing. We have mechanisms set up to select the best research and we'll discuss that today. It isn't perfect. But nevertheless, there's an accepted mechanism for selecting what's best. It is a far more difficult question to ask how much chemistry should we do versus computer science, or how much particle physics should we do versus cellular biology. Those are the kind of questions that I think we don't handle too well. Again, I can't think of a better place to be worrying about that type of question than this committee.

Those are my introductory remarks. Now I'll get to what you asked me to talk about today.

I can't add much to what I've already said in my written remarks. I told you there that I think peer review of some kind is absolutely essential, because peer review is nothing more than getting the judgment of many smart people with different viewpoints. I don't care whether it's science or politics or any other field; it's always good to listen to bright people who have different viewpoints. So I think, you know, it's impossible to run the science enterprise without some sort of peer review.

It's used very heavily by the Department of Energy in a way that you might not have thought of, and that is the allocation of the very expensive research facilities which the Department owns and operates. The Department of Energy certainly must run the biggest assembly of expensive facilities of any agency in the Federal Government, all the way from the Fermi National Accelerator Laboratory—which is a couple hundred million a year operation—to a lot of other accelerators, synchrotron light sources, nuclear research reactors, electron microscopes—a vast array of expensive facilities. The time on them is allocated by a peer review process.

So I think the Department uses the peer review process as much in this respect as any other way. In many cases, for example, the time on a beam time at Fermilab, you're talking about thousands and thousands of dollars an hour. So you have to allocate that with great care.

It's an enormous consumer of the time of good people, and this is pointed out in your letter to me. I can't do any more than repeat it. The good scientist is hit pretty hard. He is asked to evaluate proposals, to do science him or herself, and then referee the articles for the journal afterward. That's a lot of paperwork that your good scientist does. So anything we can do to ease this burden and keep the quality up at the same time is worth doing.

It works best for the disciplines. Now, from now on, this is just Jim Kane's opinion. I think it's excellent for the disciplines. Within a discipline, the best scientists are certainly the ones qualified to judge work. It has its problems working cross discipines, and it's absolutely ineffective at working totally across. I don't mean an interdisciplinary process or interdisciplinary project that's being evaluated; what I mean is you cannot ask the peer review system to do what I said a minute ago—evaluate a project in high energy physics and compare it with one in materials science. So there are limitations to the peer review process.

It has some problems, which I went into in my written testimony. There is nothing new there. I'm sure you've heard it from others. The problems are that sometimes it doesn't handle interdisciplinary proposals too well. It can be hard on young investigators because they don't have their track record established yet. And it can be hard on unconventional proposals. I don't think that's a fatal flaw. I just think it takes care. As I said in my written remarks, the answer to most of these is just a greater care in selecting members of the peer review panel. Get people who are known to be receptive to wild ideas; get people who are knowledgeable about interdisciplinary work to act as your peer reviewer.

Finally, you asked me if there was anything I could suggest that would fix this procedure. I only came up—other than get good people with good judgment, which is a kind of a nothing recommendation—with one, and that is, I think the grant period should be extended. I made a point in my written remarks that we've gone too far in this idea of procurement. Today you read a proposal and the contract or the grant that's written, and then the auditing is done. It sounds like we're doing piece work of some kind. I think that's very bad. I think we should kind of go back to the original premise. What we're buying are the brains of the smartest people in the business, and as I said in my written remarks, it seems ridiculous we have to run a quality check on these brains every year or so when the contract is renewed. So I think it would be an excellent idea to have the grant period a nominal 5 years—contingent on the availability of appropriated funds, but a longer period of review. And in the interim, rather than writing proposals, to have the individual submit the papers that he or she has published during this time. That's really the best. The papers presumably have survived a tougher refereeing process and they have or have not gotten the author tenure or whatever the author is striving for. So the papers are where the author is apt to put a lot of time and effort.

I think that's my one constructive remark, which is that we ought to look very hard at extending the contract period, and to make the conditions for renewal even after 5 years a little less rigorous.

My final remark, and then I'll pass it on to my fellow panelists here, is that lot of people have the habit of associating peer review with one kind of peer review, and that is prospective—what I call prospective peer review. I think the science community is into the strange habit of evaluating promise more than it has results. There are many other kinds of peer review—and I made this point in my written remarks. One of the best kind is retrospective, what actually happened, rather than what was promised. So when I say peer review, I do not just mean a prospective peer review based on promises and estimates, but I mean a peer review of progress as well.

The final thing—I've said "final" twice. I'd better quit—is that I am a great believer in some discretionary funds at some level. I could give you many examples of projects which did not survive the peer review process but were saved by a very perceptive individual that had some discretionary money. I don't know how to recommend this. I know the national labs have a procedure. They have a procedure for the director getting discretionary money, where he can support an individual who somehow or other has failed to convince the external world that his research, his or her research, should be supported. So in some way or other, I think discretionary money at some level—not a large amount, but at least some—would help to patch up some of the holes that the peer review process occasionally shows.

But, on the whole, I think it's working well. It's a good process. And rather than abolish it, we should try to fix its problems.

Thank you.

[The prepared statement of Dr. Kane follows:]

USE OF THE PEER REVIEW PROCESS IN THE DEPARTMENT OF ENERGY

JAMES S. KANE

Mr. Chairman and members of the Committee, thank you for allowing me to testify before your Committee on this very important topic. As you know, I no longer work for the Department of Energy. So rather than a current description of the Department's operations, you will instead get my impressions of how peer review is used in the Department's research programs. These impressions have been gained through over 30 years of association with the DOE and its predecessor agencies. Much of this time was at a National laboratory, some of it in Washington as a science administrator, and now I am at the University of California, an institution that, in addition to its outstanding reputation for research, is also the manager of three of the DOE laboratories.

I will frame my comments on peer review around answers to the following questions: How is the process applied within the DOE? How well is it working? What are its virtues? What are its problems? What might be done differently?

You will notice that I didn't question the need for peer review, in some form. I can't imagine the science enterprise of the United States being done without the benefit of many viewpoints, and without the advice and judgment of the best minds in the various fields. Science has done and is doing well in the United States, and I believe peer review is one of the reasons. Success, both past and future, depends on finding and rewarding the best, and this selection process should be based on the collective opinions of experts who are themselves the best in the fields under consideration. If an organization administering research does nothing more, it will succeed if it identifies the top ten percent of the scientists working in a field, and sees to it that they are allowed to do whatever their inclinations tell them. So the process of peer review is a tool that enables science administrators to find the best brains and to support the most fruitful science. Like any other tool, however, it must be used with skill.

Those are my feelings on peer review in general. From now on, I intend to address the use of peer review in the Department of Energy, and more specifically, in the Office of Energy Research of the DOE. I can justify this selection by pointing out that Energy Research supports perhaps 90% of the basic research of the Department, and certainly more than 90% of the university research funded by the DOE. Energy Research is a very large funder of research, even by federal standards. Its annual research budget exceeds $1.8 billion, most of it basic research, the remainder being highly sophisticated applied science. Energy Research is also the principal organization of the federal government in the construction and operation of very large facilities used for basic research--accelerators, including the ones at Fermilab and at the Stanford Linear Accelerator Center, nuclear research reactors, synchrotron light sources and other large and expensive scientific facilities. So peer review, in Energy Research, is used not only to choose the scientists that are to be funded, it is used in addition to choose the experiments to be done on these expensive facilities, whose operating costs can be thousands of dollars an hour. I should like to point out that the large research facilities are used predominantly by university scientists---in the case of the high energy particle accelerators, for example, the university community comprises about 75% of the users.

Before I start describing how Energy Research uses peer review, let me first define what I mean by this term. I can identify at least five different kinds of peer review, all of them used to varying degrees by Energy Research:

1) Mail reviews of written proposals, done individually by several (usually 5 or 6) experts. The proposer does not know the identity of the reviewers. These reviews are prospective, in that they evaluate promise, not performance, although the past record of the proposer is invariably considered.

2) Panel reviews, based on written proposals evaluated by a panel of several experts, which collectively rank-orders the proposals. These reviews, too, are prospective.

3) Oral panel reviews, in which a panel of experts evaluates a proposal, usually on the basis of both written and oral presentations. These too, are prospective.

4) Progress evaluations of individual research projects, usually by an appointed panel of experts, with a presentation by the project scientist. These reviews are retrospective. Progress, rather than promise, is evaluated.

5) Evaluation of group efforts, by a panel of experts. Again, the review is retrospective. The evaluation is focused on accomplishments and progress.

As I stated above, Energy Research uses all of these forms of peer review. The first three are the most common for research sponsored at universities, and the latter two for research at the National Laboratories, but there are occasions where process 1) is applied to work at the Laboratories, and process 5) is applied to work at universities. I think nearly every basic research project sponsored by the DOE is subjected to one or more of these processes.

I mentioned earlier that the allocation of time on the expensive research facilities at the DOE laboratories is also done by the peer review process. Each research facility has a program committee of expert scientists. A majority of the committee is from institutions other than the one at which the facility is located. This committee reviews all proposals that involve use of the facility, and rank-orders them by merit. This procedure ensures that scientists from the site where the facility is located do not have an unfair advantage in gaining access to the facility. Most of the scientists doing experiments at a facility are from other institutions.

Having endorsed whole-heartedly the concept of peer review, and having also described in very general terms how it is applied to DOE research, let me now make some comments, all of them judgmental on my part.

o I believe that peer review works best for programs within the scientific disciplines, and is most difficult for applied and interdisciplinary work. Nevertheless, it should be used, in some form, for all kinds of research.

o Peer review has problems, and finding ways to correct or alleviate these problems should be an important goal of the scientific community, as well as this committee. I'm sure other witnesses have pointed this out to you.

o Peer review is enormously demanding of time and effort by the very people who should be spending their time doing science. I occasionally wonder if the process, as we now apply it, makes sense. As long as the very best people receive support, is it so important that all proposals be evaluated in excruciating detail, often by these same top people? So a high priority should be to make the process less burdensome. I will comment on that later.

o Prospective peer review can be very hard on young scientists just starting their careers. Without a track record, its often difficult to get that first grant.

o Prospective peer review, unless done with great care, will often reject good projects that are interdisciplinary, or fall between the disciplines. No one knows how often this happens, because if a project doesn't get funded, its merits may never be known. But there are enough examples to cause concern. Incidentally, most of the happy endings, and by that I mean projects rejected by peer review that are funded by other means and prove successful, came about through discretionary funding, i.e. an individual at the local level with access to funds circumvented the peer review process.

o Peer review processes occasionally reject "far out" proposals. Again, this doesn't happen if the most insightful reviewers are chosen. But these people are a scarce asset; they are busy. No one knows how many truly outstanding ideas are rejected; there probably aren't many. But there are just enough examples to cause concern. Again, it is interesting to observe that some very unconventional research has been supported by discretionary funding at the local level. I think there is a message here.

I am the first to admit I don't have solutions to these problems with the system. But I would like to recommend some measures for your consideration that I believe would help.

I think the most important thing we could do to reduce the burden of peer review would be to increase the period of the grant, i.e. the interval between reviews. I sometimes think we forget the purpose of supporting science is to buy brains, not to enter into a contract to do a specific piece of pre-conceived research. It is hard for me to believe that brains must have their quality checked every year or so. So my first and strongest recommendation is that a proposal that gets good marks from a competently done peer review be given a five year term, subject only to availability of funds. I realize that in very unusual circumstances a principal investigator can get into trouble, personal or otherwise, and I would recommend that the government official in charge of the project be allowed to shorten the grant term in such extraordinary circumstances. I don't believe this would happen often.

The problem of scientists not yet established in their careers also should have attention. One approach is to have a special procedure set up, with earmarked funds, to reward promising scientists early in their careers. The High Energy Physics Program in DOE has such a effort, as do several other agencies that support basic research. (I am intentionally avoiding the word young, because its probably illegal, but in fact, any outstanding scientist not yet recognized is almost sure to be young.) Some sort of provision for the judicious allocation of discretionary funds at the Department level is another good approach. Deans and Department Heads, as well as laboratory Directors, very likely know who their best young people are, and they have every incentive to identify and reward them. If wrong decisions are made occasionally, so be it. Picking the winners in research is never a sure thing.

It is important that we look carefully at the difficulties involved in choosing interdisciplinary projects, especially with the current emphasis on encouraging research in areas thought most likely to increase the Nation's economic competitiveness. Many of these areas will be interdisciplinary.

A serious problem with evaluating an interdisciplinary proposal is that by definition, the whole is greater than the sum of the parts. Some of the disciplines contributing may be essential to the success of the overall project, but their contribution is not truly basic research. A reviewer evaluating that aspect of the proposal from a disciplinary point of view may decide that since that particular part of the proposal isn't worthy of support, the whole proposal should be rejected. Since much interdisciplinary research is goal-oriented, rather than fundamental, reviewers should be sensitive to the importance of a successful outcome in a broader context than just advancement of the disciplines.

There is no inherent reason why peer review shouldn't work just as well for interdisciplinary evaluations as for disciplinary ones. My personal prejudice, however, is that retrospective review is better than prospective. In any event, the most important contributor to the success of the review is an appropriate choice of reviewers. In addition, officials responsible for sponsoring research should be alert for proposals that get "mixed grades". If a proposal gets enthusiastic reviews from some of the reviewers, and is rejected by others, such a proposal should get much closer scrutiny, with consideration by a more carefully selected group of reviewers. I do not believe that the difficulties associated with review of interdisciplinary projects are a major problem in the peer review system, but they are a concern. Perhaps they haven't gotten the attention they deserve.

One important problem with interdisciplinary funding is that budgets are often defined in terms of disciplines. A proposal that falls between disciplines, or more likely, involves several disciplines often loses out in competition with purely disciplinary proposals. The solution to this difficulty is mostly administrative; each funding unit must make sure it is organized in a way that does not penalize interdisciplinary projects, and must be sure that its choice of reviewers is appropriate for the topics being reviewed.

Finally, the accusation is often made that peer reviews are conservative; they tend to choose extrapolation of what is known over bold (and often wrong) ventures into the unknown. I am reluctant to say that this is a serious problem, because without more information that would be speculation on my part. But I am aware of just enough examples of research that turned out to be very good, but was initially rejected by peer review, to be concerned. Really remarkable advances in science almost invariably come from revolutionary ideas, so if the process of peer review rejects, even rarely, high risk, high payoff research, that is a very serious defect.

I can mention a couple of ways that might be used to reduce the probability that good science will be denied funding because of its unconventionality. Both involve more attention at the administrative level. I have mentioned the first of these earlier: allow discretionary funds at the local level. This system is already in effect at the National Laboratories. Each Director has funds for the express purpose of supporting research that is thought to be meritorious, but has somehow been unable to get funded through the more conventional channels.

Another way that has been used in the Office of Energy Research, and in my opinion used very successfully, is to set up a special organization to handle the unconventional proposals. In the DOE this is called the Office of Advanced Energy Projects. Locally, however, it is known as "Infants and Orphans". Its purpose is simple: to fund worthwhile proposals that somehow don't fit the bureaucracy. To work well, this approach must have a very perceptive official in charge, and must have a similarly sensitive slate of peer reviewers to draw upon.

Our present pluralistic system of research support is also an excellent insurance policy to rescue proposals that for some reason are rejected by one organization's peer review. Such proposals often can get favorable consideration by another funding agency.

That about sums up what I have to say. To repeat myself, I think some form of peer review is essential. Competition works, and excellence must be rewarded. But that doesn't mean that only one kind of peer review should be used, or that we shouldn't consider some changes in the way we do business. The present system is burdensome and levies an unacceptable overhead on our most valuable resource.

Mr. Packard. Thank you, Dr. Kane.

I think we will change the procedure slightly and go right ahead and have the witnesses speak, and then we'll ask questions of all of them, if that's all right with you, Mr. Chairman. So let's move on then to Dr. Moss, who is Director of the Office of Naval Research. We sincerely appreciate your being here, Dr. Moss, and we'll now have your testimony.

Again, you may summarize or read as you wish.

[A biographical sketch of Dr. Moss follows:]

Dr. Marvin K. Moss

A native of Burlington, North Carolina, Dr. Moss obtained his undergraduate degrees in Mathematics and Physics from Elon College, and advanced degrees in Nuclear Engineering (MS) and Physics (PhD) from North Carolina State University. As Director of the Office of Naval Research (ONR), he is the senior civilian line manager of the agency responsible for the direction and management of the Navy's $371 million basic research appropriation. ONR is composed of seven major directorates, three laboratories, plus an environmental prediction research facility, two overseas detachments (London and Tokyo), and the new Institute of Naval Oceanography. The directorates consist of research programs, technology programs, oceans science and technology programs, university business affairs, acquisitions and Navy patent programs.

Dr. Moss was appointed Director of the Office of Naval Research in June 1985. He has been instrumental in the development and refinement of an ONR corporate process to effectively plan future research investments and develop a more responsive organizational structure. Dr. Moss initially joined the Office of the Chief of Naval Research (OCNR) in 1982 as Director, Planning and Assessment and Deputy Technical Director, and served as Technical Director between August 1983 and June 1985.

Dr. Moss come to OCNR from the Department of Energy (DOE) where he was Associate Director of Energy Research. He was involved with numerous aspects of energy science, technology and policy, and in the international ramifications of nuclear energy and with other basic energy issues. He also worked with invironmental research programs and served as the principal DOE coordinator for acid rain research. During 1978–1980, Dr. Moss was active in the President's International Nuclear Fuel Cycle Evaluation, a program involving countries and scientists worldwide. He joined the government in 1975 and served for two years with the U.S. Arms Control and Disarmament agency, where he became Director of the Nuclear Division.

Dr. Moss has chaired a number of policy formulation groups under the President's National Security council, coodinated numerous scientific and policy-related studies for the State Department and the DOE, and represented the United States and DOE in energy-policy negotiations with other countries.

Prior to joining the government, Dr. Moss spent 16 years in basic research and teaching at North Carolina State University, attaining the rank of Professor of Physics in 1971. While at the University he was active in research programs in theoretical physics, general relativity and cosmology, laser physics and energy systems. He also tought at the graduate level in the departments of mathematics and nuclear engineering.

Dr. Moss was cited as a Presidential Rank Executive for 1985, recognizing his extraordinary contributions in bringing outside expertise into OCNR to reform the ONR research programming process, and establishing new research management organizations for Naval Ocean Science and Technology.

Dr. Moss holds several other government and academic awards, and is the author of many scholarly works in the areas of theoretical physics, general relativity, laser physics and spectroscopy.

STATEMENT OF DR. MARVIN K. MOSS, DIRECTOR, OFFICE OF NAVAL RESEARCH, U.S. DEPARTMENT OF DEFENSE, ARLINGTON, VA

Dr. Moss. Thank you very much, Mr. Chairman. I have submitted written testimony for the record.

What I would like to do this morning, with your permission, is to supplement that to a degree and summarize to a degree. Our process for contract selection at ONR I think is different and, to really establish the flavor of that and how it works, I need to move into a couple of areas that are very integral to the process, per se, because we do try to prioritize there. We're mission-oriented and, as I say, we do things a little different.

I do pick up on the comment that Mr. Kane made here, that we're really looking at science after 40 years. The Office of Naval Research is having its 40th birthday this year, come August the 1st. I think we started that process after the end of the last war, and it is a real pleasure to be here to tell you a little bit about the way we do business.

I have supplied handouts from which my comments may be followed. I state our mission (chart 1), which is to support research, but to do that for the maintenance of naval power and for national security.

Our goals (chart 2) are also given there and the point that I want to make here is in the middle of that chart. It indicates four major areas where we do research: physical and mathematical sciences, life sciences, engineering sciences, and environmental sciences. By environmental we mean the Navy's environment that is mostly ocean science, oceanography, geophysics to a degree.

We support 15 disciplines in those four categories, from the hard sciences of math, physics, chemistry, electronics, to the life sciences, the biotech, biology, even psychology and training, which is very important to the technological Navy today.

The people that do this work for us are indicated on the next chart (chart 3). To the left we have eight claimants, we call them, in ONR. The one that is known by really the outside community is our contract research program. That's about two-thirds of our dollars, as you'll see from that pie chart. And over 80 percent of those dollars go to the universities. Our other programs go to our own corporate laboratories, the Naval Research Laboratory across the river from National Airport, which I think is probably one of the most prestigious, if not the most prestigious, laboratories in the country or in the world. And also, all of the Navy R&D centers scattered around the country, from Panama City, Florida to Massachusetts, and from the east coast to the west coast.

To the right there you'll see that universities in this country receive 56 percent of our funds. We anticipate that that will increase with the addition that Congress gave DOD for the University Research Initiative. Those numbers are not reflected there because they actually have not come to us as yet. But we're in the process of beginning to make selections for URI and the money is on its way, I understand.

What do these people do is the next question I would just like to briefly mention. And chart 4 shows, for example, a problem that occurred back in the early sixties. Aluminum, which was used in most all aircraft—is a metal that's strong, and lightweight—a particularly good material for Navy platforms, for commercial aircraft. But there was a problem with stress corrosion cracking. To get around this problem of cracking, the aluminum would have to be heated, more or less to fuse the cracks, but which actually would reduce the strength. You would have to use a thicker section, and hence the weight went up. ONR started a research program in the mid-sixties to develop a better aluminum alloy. We worked with the Naval Air Systems Command, one of our eight claimants. They worked with the Naval Air Development Center at Warminster, PA, who later worked with Alcoa in the private sector, who brought the research scientists. There were also some university scientists involved, and the Navy labs and the private sector developed the alloy 7050. If you look at the chart, you'll see the increased strength with smaller sections of it.

This alloy, for example, was heralded at the time—and I think this is still true today—as the most useful alloy developed in the history of the aerospace industry. Most every aircraft and missile today uses 7050 and it is retrofitted in the F-18's, the Poseidon missiles, the A-6, the S-3A, and other aircraft in a number of ways. But this is the type of thing we do, and this is the type of thing that ONR has been involved in over the years.

Our funds for research, as I indicated earlier, are in most every major field of science (chart 5). We have 15 disciplines that we fund. We're mission-oriented, however. Consequently, we have additional programmatic constraints. Our programs must have first outstanding scientific merit. We're the basic research arm for the Navy. Our programs, however, must also fit into an overall programmatic priority to the benefit of the Navy.

How do we develop these priorities? What is their importance? Well, they are integrally related to how we select projects for funding, and so I would like to say just a word or two about how we set those priorities.

The next rather complicated chart that I have there (chart 6), a series of ovals or ellipses, indicates the process we go through to plan how to allocate our resources. It starts every year in October and it ends in June.

I would focus your attention on this busy chart just to that second column that starts with "RPPG" at the top and "Navy Needs" there in the middle. Each of those four areas there in that column represent guidance that we put out to all of our claimants. Anyone that funds research from the Office of Naval Research does a mandatory study of those documents, and they are put out every year. For example, Mr. Chairman, the "Navy Needs Document" is a classified document. But we're mission-oriented and we look and study everything that we can find to indicate what the problems in the fleet are.

For example, we look at defense guidance; we look at the maritime strategy; we look at potential strategy stoppers; we look at strategic planning documents; we look at summary warfare appraisals for about 16 different warfare areas, from ASW to mine warfare to whatever. It's difficult, looking at those and seeing the needs, and to translate these into what you need to do in the basic research area. Fleet commanders don't talk basic research language. Sometimes it's almost like translating German to English, and perhaps when you haven't even taken a course in German. It's very difficult.

We bring a group of Navy officers and our outstanding scientists together, and we sit down and we put out a classified document that is about three-quarters of an inch thick for every warfare area that the Navy is concerned with. It lists statements of basic research that needs to be done to meet those areas.

That's just one of those documents. At the top, the RPPG, that's our Research Planning and Programming Guidance. There we use the Navy needs but we also assess our own programs, our 15 disciplines. What's happened that's new, for example, in the world of physics or chemistry over the past year. What the new opportunities are. What we have been doing for a long time that needs to be cut out or reduced or leveled.

Our major help there comes with a program that I personally implemented over the past year to a year and a half. We now have, for each of our 15 disciplines, a National Academy of Sciences Board that meets, looks at our program, then goes off by itself and comes back and gives us a 10- or 15-page document on what the new opportunities are, for example, in chemistry. Our first report we received about 6 months ago was in chemistry. It got a really interesting review in the *Chemical & Engineering News*, a few months ago. It's really a document that helps us focus on where we need to go in chemistry, and we're doing that in all of our areas. As I say, these guidance areas lead us to where we need to support research.

One other comment here is that you will see a vertical dotted line there. At this point we focus our research into thrust areas or into the core areas, about 50–50. This guidance goes for all of the programs.

The thrust areas are areas where we really want to throw some large dollars behind a ripe, scientific area, where we think that it would benefit the Navy—for example, ultra-submicron electronics or nonlinear dynamics and routes to chaos today, a very new and very interesting area for science and for the Navy. The thrust areas are, almost all greater than 90 percent interdisciplinary programs, and they are programs that we really need to push to accelerate science and hopefully to the benefit of the Navy. But in prioritizing those areas is where we really focus on where our research dollars will go.

The next chart shows the participants in our peer review (chart 7). We will, for example, put funds in thrust areas to the tune of $500,000 to $2 or $3 million depending on the area that we want to focus on over, say, a 5-year period. And we do this every year. But we will bring in people from the universities, from the Government, from the operational Navy, from the other laboratories, from industry. Last year we had about 75 there. Their institutions are listed there (chart 8). And they sit on a panel. They hear our proposed thrust areas, and we have more thrust areas than we have money. We introduce a competition there. And then they prioritize and they rank for us. We basically go down the list with the dollars that we have.

So there is a peer review in ONR, but this peer review, for example, is to define an area that we really need to do research in, and there may be 10 to 20 separate principle investigators at different universities that fall under the aegis of just one of these thrust areas. As I say, most all of those are interdisciplinary.

Well, now, with this backdrop, the leading edge then of our proposal selection process comes from our scientific officers, once we have defined where we want to go (chart 9). This is the crux, as I say, of our selection process. And so I would like to expand this slightly.

Our scientific officers are not just contract monitors. Academic scientists and academic peer reviewers cannot be expected to be knowledgeable about the Navy mission and all the implications for research in the Navy. Our scientific officers cannot be passive and only react to proposals from academia. They must actively communicate ONR's programmatic interest and priorities to the university community. They must spend considerable time visiting university laboratories and scientists. They must organize special conferences and workshops. They must participate in relevant activities of the National Academy, professional societies, and other organizations. To be effective, they must establish a level of visibility and scientific nature in their respective research communities.

In addition, our scientific officers must work closely with other DOD and Federal organizations—for example, SDIO—and they must seek out mechanisms to ensure current knowledge of Navy and DOD needs and the incorporation of these needs into ONR's research program.

The job of a scientific officer at ONR is a formidable task. They are given broad discretion at ONR in the selection of external projects for support. They are held responsible for their results. Over the 40 years of our existence, ONR has had a reputation for research excellence and relevance. Over this time no formal policy requiring peer review has ever existed. We solicit advice of associates within and without the Navy. This advice may be solicited informally or formally. External reviews, if employed, are of an advisory nature, and the approach for advice is determined by the scientific officer to meet the particular needs of his or her program.

As I have discussed, ONR does have a formal process for developing thrust areas to further our science and Navy research needs. Particular thrust areas may involve many university investigators, but the selection process for the thrust areas is the same as for all of our areas. It is a somewhat informal but highly conscientious process which I have just described.

Does the process work? It works for us, but we're a highly relevant, mission-oriented organization to the U.S. Navy (chart 10).

Mr. Chairman, if I could give you maybe one example, or one-and-a-half examples, if you would allow me. But on the chart there we show our universal blood program. Let me just mention that to you (chart 11). Basically, without even trying to get into a tutorial, we know there are basically three different types of blood. There is type O, the universal donor—anyone can accept O, no matter what your type is—and there's type A and there's type B. And we know the consequences if a type A is given to a type B person. You have about 24 hours for a complete blood transfusion or it's usually fatal.

But, very simplified, the A and the B differ from the O by some sugar molecules that are different from the A and the B, but attached to the blood cells. In our biotechnology program, we have developed an enzyme, or a protein, if you prefer, for type B that will literally come in like "Pac Man" and eat that sugar molecule off. We can filter it almost with a coffee filter, get the enzyme out, and we have type O blood. That's already been tested in mice today. In type A we think we have the breakthrough there.

The implications of this for the Navy aboard ship in a war, the battlefield for the Marine Corps, or the highways and the hospitals of this country, where you don't have to carry all the different types, and you don't have to worry about typing the blood before you give a transfusion because type O is hard to get are tremendous. This, again, is the type of thing that we at ONR push. There's a very definite need in DOD and the Navy for this. It's very basic science.

One other quick—my "half," if you would—shows a few front page articles over the past 6 months to a year with ONR including the discovery of the *Titanic* by my good friend Bob Ballard at Woods Hole, who has been an ONR principal investigator for a number of years (chart 12). ONR funded the development of ARGO, which was the fish that swam around and found the *Titanic,* went down and took pictures. The ARGO has been funded over about a 3-year period 100 percent out of ONR. This was the first voyage. This was the sea trial for ARGO, and I think you can deduce some of the implications for the Navy in the future of the system. And actually, this system is only half built. It's really an ARGO-Jason. Jason hasn't even been developed yet—an another ONR project.

And last, at the bottom there, the Nobel Prize for chemistry in 1985 was shared by Dr. Jerry Karle at the Naval Research Laboratory, and Herb Hauptman. Jerry Karle has been at the Naval Research Laboratory since 1945. He's been funded by ONR since 1945. His research that led to the Nobel Prize was done in the fifties. But this is the type of research that we've been funded. This is the first, to my knowledge anyway, Nobel Prize that's been given to a Government laboratory scientist outside of NIH. [Laughter.]

Mr. Chairman, as I indicated earlier, we're having our 40th birthday this year with a symposium at the National Academy in October. In sending out letters for some of our prestigious past scientists and present scientists to speak at our 2-day symposium there, I got a letter back—and there's two sentences that I would like to read to you. But I think it characterizes ONR. This is dated December 30, 1985. It says:

> Dear Dr. Moss: I've always considered ONR an example of how science should be federally supported in the most effective way. I am therefore very happy to accept your invitation to participate in the 40th anniversary symposium at the Office of Naval Research.

I appreciate being here and that ends my statement.

[Charts 1–12 of Dr. Moss's oral presentation plus his prepared statement follow:]

ONR MISSION

The Office of Naval Research was created August 1, 1946 by P. L. 588

". . . to plan, foster, and encourage scientific research in recognition of its paramount importance as related to the maintenance of future naval power, and the preservation of national security . . ."

CHART 1

GOALS

- SUSTAIN U.S. SCIENTIFIC & TECHNOLOGY SUPERIORITY FOR
  - NAVAL POWER
  - NATIONAL SECURITY
- PROVIDE SOURCE OF NEW CONCEPTS AND TECHNOLOGY OPTIONS
- SUPPORT THEORETICAL & EXPERIMENTAL RESEARCH IN
  - PHYSICAL & MATHEMATICAL SCIENCES
  - ENGINEERING SCIENCE
  - LIFE SCIENCE
  - ENVIRONMENTAL SCIENCE
- RETAIN VIGOROUS SCIENTIFIC MANPOWER & LAB BASE
- APPLY RESEARCH RESULTS IN NAVAL WARFARE AND WARFARE SUPPORT AREAS

CHART 2

DISTRIBUTION OF FY 86 NAVY BASIC RESEARCH FUNDS

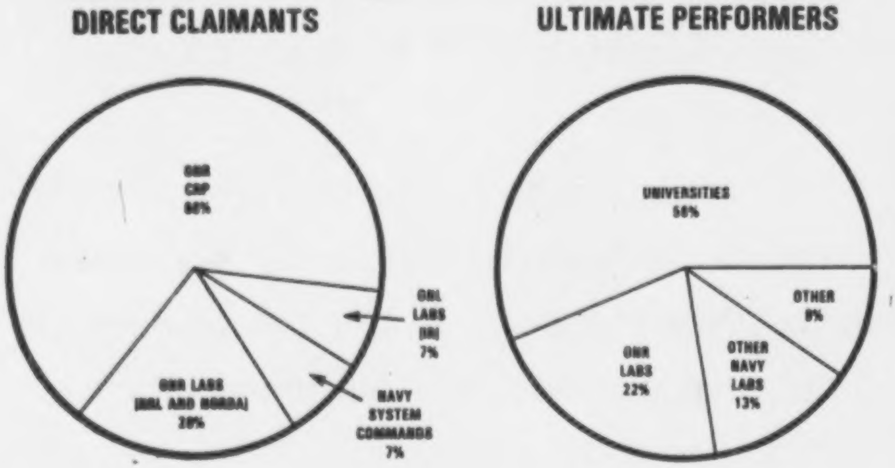
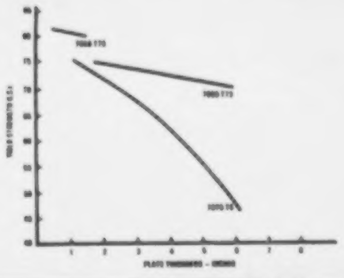


Chart 3

STRESS CORROSION RESISTANT ALUMINUM ALLOYS



OBJECTIVE:

- DEFINE FUNDAMENTAL MECHANISMS IN STRESS CORROSION CRACKING OF THICK ALUMINUM STRUCTURAL COMPONENTS IN AIRCRAFT
- DEFINE ALLOYING MATERIALS/TREATING TECHNIQUES TO IMPROVE PROPERTIES

APPROACH:

- INVESTIGATE MICROSTRUCTURES TO ESTABLISH MECHANISMS FOR STRESS/EXFOLIATION CORROSION
- INVESTIGATE ALLOYING MATERIALS FOR STRONGER MATRICES, REDUCED QUENCH SENSITIVITY, HIGHER FATIGUE STRENGTH AND TOUGHNESS

PAYOFF:

- TYPE 7050 ALUMINUM ALLOY ACCLAIMED AS MOST USEFUL ALLOY IN HISTORY OF AEROSPACE INDUSTRY
- EVERY NEW AIRCRAFT AND MISSILE USES THIS MATERIAL IN SOME FORM
- SAVES MILLIONS IN REPAIRS, LIFE OF AIRCRAFT

Chart 4

ONR FUNDS RESEARCH IN MOST EVERY MAJOR FIELD OF SCIENCE

BUT WE ARE MISSION-ORIENTED
AND, CONSEQUENTLY,
WE HAVE ADDITIONAL PROGRAMMATIC CONSTRAINTS

OUR PROGRAMS MUST HAVE OUTSTANDING SCIENTIFIC MERIT
AND
OUR PROGRAMS MUST FIT INTO OVERALL PROGRAMMATIC PRIORITIES
TO THE BENEFIT OF THE NAVY

CHART 5

ONR RESEARCH PROGRAM DEVELOPMENT

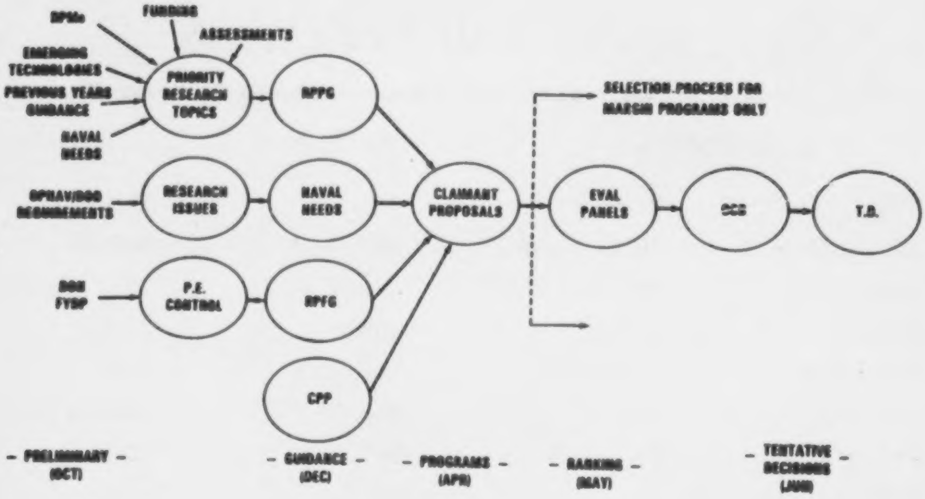

DPMs
FUNDING
ASSESSMENTS
EMERGING TECHNOLOGIES
PREVIOUS YEARS GUIDANCE
NAVAL NEEDS
PRIORITY RESEARCH TOPICS
RPPG
OPNAV/DOD REQUIREMENTS
RESEARCH ISSUES
NAVAL NEEDS
DOD FYDP
P.E. CONTROL
RPFG
CPP
CLAIMANT PROPOSALS
SELECTION PROCESS FOR MARGIN PROGRAMS ONLY
EVAL PANELS
CCB
T.D.
– PRELIMINARY – (OCT)
– GUIDANCE – (DEC)
– PROGRAMS – (APR)
– RANKING – (MAY)
– TENTATIVE DECISIONS – (JUN)

CHART 6

EVALUATION PROCESS

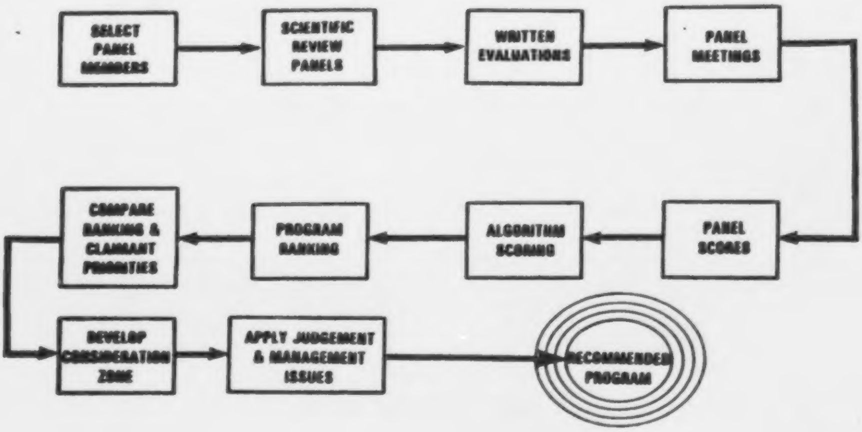

SELECT PANEL MEMBERS
SCIENTIFIC REVIEW PANELS
WRITTEN EVALUATIONS
PANEL MEETINGS
PANEL SCORES
ALGORITHM SCORING
PROGRAM RANKING
COMPARE RANKING & CLAIMANT PRIORITIES
DEVELOP CONSIDERATION ZONE
APPLY JUDGEMENT & MANAGEMENT ISSUES
RECOMMENDED PROGRAM

CHART 7

SOURCES OF REVIEWERS

UNIVERSITIES

YALE – 2
COLUMBIA – 2
PENN – 2
BROWN
UCB (RETD)
MIT – 2
JHU – 2
SUNY
NYU
PENN STATE
BROOK. POLY.
MARYLAND – 5
RHODE ISLAND
COLORADO
ILLINOIS
SO. CAROLINA
VPI
DREXEL
TEXAS A&M
GWU
N. C. STATE

GOVERNMENT

ONR – 3
ONT – 7
OPNAV – 7
USNA
NBS – 5
NSF
NIH – 2
EPA
DOE – 2
NASA
DARPA
NOAA
ARO
AFOSR

INDUSTRY

BELL LABS –
DUPONT
WESTINGHOUS
GE
ORNL
LINCOLN LABS
NCAR
WHOI

CHART 8

THE LEADING EDGE OF OUR PROPOSAL SELECTION PROCESS IS OUR SCIENTIFIC OFFICERS.

Chart 9

DOES THE PROCESS WORK?

IT WORKS FOR US -- A HIGHLY RELEVANT MISSION-ORIENTED ORGANIZATION
TO THE U.S.NAVY

EXAMPLES

Chart 10

BIOMOLECULAR ENGINEERING

UNIVERSAL BLOOD PROGRAM

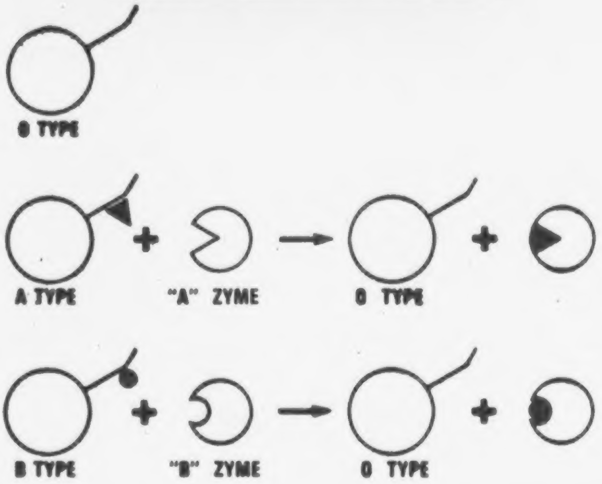

O TYPE
A TYPE
"A" ZYME
O TYPE
B TYPE
"B" ZYME
O TYPE

Chart 11

6.1 FRONT PAGE PRODUCTS
(Tip of the Iceberg)

TITANIC/ARGO
- DR. ROBERT BALLARD
- TITANIC

OCNR SCIENTISTS IN SPACE
- PAUL SCULLY–POWER
- JOHN DAVID BARTOE

1985 NOBEL PRIZE
- DR. JEROME KARLE
- DIRECT METHODS FOR THE DETERMINATION OF CRYSTAL STRUCTURES

Chart 12

Statement of

DR. MARVIN K. MOSS

Director, Office of Naval Research

before the

SCIENCE POLICY TASK FORCE

House Committee on Science and Technology

on

Research Project Selection

9 April 1986

COMMITTEE ON SCIENCE AND TECHNOLOGY
SCIENCE POLICY TASK FORCE
HEARING ON
"RESEARCH PROJECT SELECTION"

APRIL 9, 1986
STATEMENT OF
MARVIN K. MOSS, PH.D.

Good morning. My name is Marvin K. Moss. As Director of the Office of Naval Research, I am particularly pleased to have the opportunity to address you this morning. This year marks the 40th Anniversary of ONR. Our organization was established by the Congress on 1 August 1946 as a direct result of a previous comprehensive assessment of national science policy conducted by Vannever Bush at the end of World War II. As the first federal organization to sponsor a broadly based program of fundamental scientific research, ONR developed policies and procedures forty years ago which became the organizational models for the National Science Foundation and other research agencies and in many ways set a tone for government, academia and industrial relationships that was subsequently adopted by others who followed in ONR's footsteps. ONR continues to manage the Navy's scientific research resources, maintains liaison with the scientific community both in this country and abroad, and supports research in nearly every major field of science. The past record of ONR makes me keenly aware of the responsibility I have in continuing its proud tradition.

ONR sponsors all of the Navy's basic research. Funds are allocated by ONR management to six internal Navy organizations, usually referred to as claimants. The six claimants to Navy research funds are the ONR Contract Research Program (CRP), the ONR Contract Technology Program, the Naval Research Laboratory (NRL), the Naval Ocean Research and Development Activity (NORDA), the Naval Facilities Engineering Command (NAVFAC) and the Naval Medical Command (NAVMED). Some claimant organizations, such as ONR's own Contract Research Program, have no in-house research capability and use the funds allocated to them to sponsor research at universities and other external research organizations. Other claimant organizations, such as NRL and NORDA, use the bulk of their allocations to support in-house research. After specific project selections have been made by the claimant organizations, roughly 56% of the Navy's basic research funds are used to support research at universities, an additional 36% supports research performed at Naval laboratories and the remaining 8% supports research at industrial and non-profit research organizations. We anticipate that with the new DOD University Research Initiative funded by the Congress this year the percent going to universities will increase significantly.

Although I will tend to discuss project selection from the point of view of contracts with universities, most of my comments will apply equally well to in-house research.

Any discussion of ONR's project selection process must recognize the fact that ONR is a mission oriented sponsor of basic research. It encourages the acquisition of fundamental knowledge needed to solve future military problems in areas such as communications, surveillance, targeting, propulsion, mobility, guidance and control, navigation, energy conversion, materials and structures, and personnel support. The Navy of the 21st Century is being planned now through the research supported by ONR.

Because of ONR's mission orientation, project selection must be a two step process. First, it must establish broad programmatic thrusts and priorities reflecting a suitable balance between Navy need and relevant scientific opportunity. Next it must select specific research projects and tasks to implement those broad thrusts and priorities. In my remarks this morning, I will address our approach to both of these essential steps in the project selection process.

I would also like to note that, partially because of its unique mission, ONR depends primarily on its Scientific Officers for the selection of specific research projects. Academic peer reviewers cannot be expected to be knowledgeable about the Naval mission and its research implications. It is ONR's exceptional cadre of Scientific Officers who made its past record of achievement possible. ONR Scientific Officers are encouraged, as a matter of policy, to be active researchers and to play a leadership role in the scientific community while, at the same time, establishing and maintaining close communication with the Navy technology development and operations communities who will ultimately use the products of their research programs. I will say more about the Scientific Officers and the responsibility and flexibility delegated to them when I discuss our project selection process in detail. But first, I would like to say a few words about the establishment of the the broad programmatic thrusts and priorities that provide the context for project selection in a mission oriented agency.

Establishment of Broad Programmatic Thrusts and Priorities

In addition to conventional guidance on budget ceilings and procedures, ONR issues two annual guidance documents to its claimant organizations that are relevant to the establishment of programmatic thrusts and priorities: the ONR Naval Needs Document and the Research Planning and Programming Guidance Document.

The ONR Naval Needs Document is a classified guidance document summarizing long term naval technology requirements. It is updated annually by a panel of technically trained naval officers to reflect the most recent maritime strategies, warfare area appraisals, master plans and other Naval needs source documents.

The Research Planning and Programming Guidance Document highlights priority research topics identified from a variety of sources, including the Naval Needs Document. A major input considered when selecting our priority research topics is provided by in-house experts in each discipline of science and engineering. The total ONR research program is divided into fifteen technical disciplines which we refer to as subelements. To provide some indication of the breadth of our program, let me identify them: physics, chemistry, mathematics, radiation sciences, computer sciences, electronics, materials, mechanics, energy conversion, ocean sciences, ocean geophysics, atmospheric sciences, astronomy and astrophysics, biological and medical sciences and behavioral sciences. For each of these subelements, or disciplines, we have identified a senior scientific officer with exceptional status in his or her field who serves the Chief of Naval Research and myself in many planning and communication roles. This individual is referred to as a Subelement Monitor. Development of the Research Planning and Programming Guidance Document begins with the Subelement Monitors producing written status reports on their discipline reflecting not only their own views but those of other experts in the field, internal and external to ONR. These very important internal inputs are supplemented by a review of relevant documents prepared by other organizations such as the National Science Foundation. In addition, we are establishing National Academy of Sciences (NAS) Panels for each of our disciplines. The NAS panels will provide us with a report identifying and discussing the priority research areas in their field as seen from their perspective. The ONR/NAS reports on priority research topics will be updated on a three year cycle. We have just received the first of these NAS panel reports. The discipline happened to be chemistry, and we are delighted with the results which received a very positive assessment in the American Chemical Society publication, C&E News.

I want to emphasize that all of these inputs, and the guidance document that results from their integration, focus on the establishment of broad programmatic priorities, not project selection. However, they do establish the context for project selection at ONR. The summary document and the source documents are mandatory review material for all of our Scientific Officers and technical managers.

I would also like to emphasize that both the Naval Needs Document and the Research Planning and Programming Guidance Document are just that, guidance documents. Once again, ONR places a great deal of responsibility on its Scientific Officers to very aggressively seek out relevant scientific opportunities and Naval requirements and to seek an appropriate programmatic balance in their areas of research.

Roughly 60% of the ONR research budget is identified as core dollars which are distributed to the Navy organizations who are claimants to research dollars on an essentially non-competitive basis. I hasten to add that the process used by each claimant organization to allocate core dollars to specific research projects is highly competitive. Priorities and thrusts for the utilization of these dollars are established by the claimant organizations subject to annual program review. The claimants establish their priorities using the guidance provided by ONR and through their own internal procedures for identifying an appropriate balance between scientific opportunity and Naval need. These core dollars are used to provide stable support for long term evolutionary research across all of the disciplines of science and engineering relevant to naval requirements. They also serve as a source of funds that can be used by scientific officers to provide rapid response to new opportunities or to at times take a flyer in a high risk/high potential payoff research area.

Additional resources are available to the claimants through the support of Accelerated Research Initiatives (ARI's) that are selected on a competitive basis at the corporate level. Research Options proposed by the claimants as candidate ARI's represent programmatic areas of exceptional opportunity where a concentration of funding of limited duration, typically five years, can yield exceptional benefits from the point of view of advancing fundamental understanding or from the point of view of the transition of new ideas into development and application. Ideas for candidate Research Options are normally initiated by our Scientific Officers. Although the exact amount varies from year to year, roughly 8% of the ONR research budget is set aside each year to fund new ARI's that are selected in a rather structured competitive process that includes inputs from expert panels. Because of the 5 year average life time for ARI's, roughly 40% of the total Navy research program is devoted to ARI's at any given time. ARIs are implemented by the responsible claimant organization(s) who define specific tasks and select contractors and in-house researchers to perform those tasks. I will have more to say about the process for selecting specific projects in just a few minutes.

In any given year, from 20 to 40 Research Options are received from the claimant organizations as candidates for ARI funding. The proposed Research Options are sorted into groups of 4 to 6 options involving related areas of science. A panel of experts is formed to evaluate each group of Research Options. Since the panels are asked to address research quality, transition potential, and Navy relevance for each Research Option, panel members selected include experts in these three areas for each option. Typically, the research experts come primarily from the university, but also include government and industrial laboratories; transition experts come from the Office of Naval Technology and the Naval Systems Commands; and relevance experts come from the Naval Systems Commands and the Chief of Naval Operations staff. Last year, 75 scientists and engineers from other organizations participated in the ONR Research Option evaluation process.

As already noted, to the fullest extent possible Research Options are grouped on the basis of related science. However, the proposed Research Options typically cover the entire spectrum of science and engineering sponsored by ONR. The group of proposals considered by each panelist will include some options that do not fall directly into his or her specific area of expertise. In the last analysis, the allocation of scarce research dollars involves the establishment of priorities across the entire spectrum of science and engineering. Panel members are asked to contribute their specific expertise and their broad experience as scientists and engineers to assist ONR in that priority setting process.

Panel members, who receive background technical information on proposed Research Options and have an opportunity for discussion with the responsible Scientific Officers and each other, rank all of the options in their area of science in priority order. After the incorporation of considerations such as overall program balance and naval priorities, a prioritized list of all proposed ARIs is developed by the ONR planning staff and myself and presented to the ONR Corporate Board for final selection.

That completes my discussion of approaches used by ONR to identify broad progrmmatic thrusts and priorities. I would like to turn to the subject of individual project selection.

Selection of Individual Projects

The ONR program employs a variety of mechanisms to support research at universities. In some cases, special objectives are included in the selection process. For example, we recently established a program to identify and support exceptionally promising young investigators. Other special programs are designed to foster substantive cooperation and interaction between research personnel at Naval laboratories and their counterparts at universities. However, the vast majority of our program is implemented through the mechanism of project contracts with individual principal investigators for research projects that are selected on the basis of scientific quality considered in the context of Navy need.

The ONR Scientific Officers play the key role in project selection and management. They are given broad discretion in the selection of external projects for support, and are then held responsible for their results. Although there is no formal peer review process at ONR, the Scientific Officers do seek the advice of associates within the Navy and of appropriate experts external to the Navy. The methods employed to seek expert advise, which may be highly structured or informal, are determined by the Scientific Officer to meet the particular needs of his or her program. External reviews, if employed, are advisory. The Scientific Officers are ultimately responsible for a project's contribution to Navy goals. The decisions of the Scientific Officers are reviewed by Division Directors and by appropriate science management. However, those decisions are rarely reversed. The ONR Scientific Officers are heavily involved in project monitoring, including site visits, and it is they who must answer for their programs in periodic program review and evaluation by upper management.

The ONR program that has resulted from the processes that I have just outlined has the following characteristics:

-60 to 65% long term research to advance the state of knowledge across the spectrum of disciplines relevant to the long term Navy needs.

-15% focused on long term high-risk efforts with high potential payoff.

-20-25% devoted to programs closely associated with specific applications with defined transition potential.

Because of the requirement to select programs that have outstanding scientific merit and fit into an overall set of programmatic priorities, the ONR Scientific Officers cannot be passive and simply react to proposals as received from the academic community. They must play a very active role in communicating ONR's programmatic interests and priorities to the academic community and in seeking out scientific opportunities relevant to Navy priorities. Not only do they spend considerable time visiting university laboratories and scientists for this purpose, they also organize special conferences and workshops and monitor and participate in relevant activities of the National Academy of Sciences, professional societies and other organizations. To do this effectively, they must have established a certain level of visibility and stature in their research communities.

On the opposite side of the coin, the Scientific Officers very actively seek out mechanisms to ensure current knowledge of Navy and DOD needs. To cite just a few examples, many of our Scientific Officers take on responsibilities beyond their principal role as Navy research managers to also manage Navy development programs, and programs of DARPA and SDIO. In addition, they consult on special near term problems faced by Naval fleet and operations personnel.

Before completing my remarks on the topic of project selection, I would like to describe somewhat more structured approaches currently employed for two special purpose programs that represent a relatively small but important fraction of our total program. I am referring to the ONR Young Investigators Program and the ONR URI Block Research Program. Both programs are components of DOD's University Research Initiative established in FY-86 to strengthen the ability of U.S. universities to perform basic research and to train scientists and engineers in areas vitally important to the national defense.

The ONR Young Investigator Program is intended to attract the best and brightest young academic researchers to areas of research that are important to future Navy requirements by providing special recognition and stable funding for research at a level consistent with the purchase of equipment and support of graduate students. The goal of the program is to provide a future resource of informed, committed scientists and engineers, and to establish strong long-term ties between DOD and outstanding academics. It must be clear that the proposed investigator is a potential leading faculty member with a strong commitment from the university's officials. Base funding for each young investigator is $50,000 per year for three years. As an incentive for becoming involved with such Navy research activities as the Navy laboratories and the Navy Systems Commands, ONR will match on a 2-for-1 basis support gained from these sources. ONR matching funds are limited to $80,000, but this does not prohibit a young investigator from getting more than $40,000 from other Navy sources. .

It is often claimed that young investigators sometimes are placed at a disadvantage by "peer" and other conventional project selection processes. We wish to insure that particularly promising bright candidates receive appropriate recognition and encouragement. The program is announced via a specific program announcement. One hundred and seventy-nine proposals were received in response to our FY-85 announcement. Unfortunately, we were only able to fund ten.

All Young Investigator proposals are initially screened by the directors of the ONR divisions responsible for the research area identified by the candidate young investigator. Division directors in turn present the case for their most promising candidates to a selection panel composed of ONR's Research Advisory Board and myself. Young Investigator proposals are scored on the basis of creativity of the proposal, past performance of the candidate, and evidence of excellence and commitment on the part of the candidate's university. Consideration is also given to a desire to have a reasonable balance of young investigators across Navy relevant areas of science and engineering.

Block Research Programs. As already noted, ONR has traditionally emphasized the individual project contract. Broader institutional support mechanisms have been employed, particularly in the ocean sciences, but even in those cases institutional proposals are evaluated essentially on a project-by-project basis. In FY 1986, under our newly established University Research Initiative, we will begin to support a number of new block research programs at U.S. universities.

The Navy URI block research programs are designed to increase the number of science and engineering graduate students; to increase investment in major pieces of research equipment at universities; to increase the investment in higher risk basic scientific research in support of critical Navy and DOD technologies; and to provide more opportunities for contacts between universities, industry, and Navy and other DOD laboratories to maximize the benefits to be derived from defense research for the nation's security, both military and economic. We recently issued a broad area announcement, jointly with DARPA, that requests proposals in 20 broad areas of science and technology critical to the DOD mission ranging from mathematics to arctic science and engineering.

The ONR block research program contracts will have a maximum life of five years with maximum annual funding of four million dollars per year. The block research program selection process can be summarized as follows:

o A proposal evaluation panel will be established for each of the research areas identified in the ONR/DARPA broad agency announcement.

o Each proposal evaluation panel will be chaired by an appropriate ONR Scientific Officer and/or DARPA Program Manager who will ultimately be responsible for recommendations and management of the block research program(s) awarded in the subject research area. Other members of the evaluation panels will include appropriate representatives from in-house Naval laboratories, the Army, Air Force, DARPA, NSF and industrial research organizations. Academics may be included when it is clear that conflicts of interest are not present. Where appropriate, other interested government agencies may be represented (e.g. NOAA or NASA in ocean remote sensing).

o Evaluation panels will review and discuss all proposals received in their subject areas and rank them into three categories. Site visits (typically 1 1/2 days in duration) will be made for all proposals in the highest category. Upon completion of site visits, each of the proposal evaluation panels will make recommendations to a Program Selection Board consisting of ONR's Research Advisory Board supplemented by representatives from DARPA, Army, Air Force, DUSR&AT and appropriate Directors of Navy Laboratories/R&D Centers and the Director, ONR.

o Final selection will be made by the Program Selection Board.

o All selections will be reviewed by an appropriate DOD committee prior to announcement to ensure coordination of Army, Air Force, DARPA, and Navy block research programs.

Summary

In order to satisfy the research objectives of DOD and the Navy, ONR uses a number of mechanisms to sponsor research. Historically, greatest emphasis has been placed on the individual project contract which we still believe represents the best mechanism for supporting work of the highest scientific quality within the context of naval requirements. Partially because of its unique mission, ONR has placed less emphasis on formal peer review in its selection process than some other research sponsors. However, the key to the ONR project selection process has always been the ONR Scientific Officer, who is a state-of-the-art scientist or engineer, not a contract administrator. The ONR Scientific Officer must be an activist with a broad knowledge of his or her discipline and an understanding of its relevance to the naval mission. ONR's record of accomplishment is more the direct result of the outstanding quality of its Scientific Officers, and the authority given to them, than the result of any particular process for project review and selection.

Mr. FUQUA [presiding]. Thank you very much, Dr. Moss.

Now we will hear from Dr. Mary Clutter, Special Assistant to the Director of the National Science Foundation.

Dr. Clutter.

[A biographical sketch of Dr. Clutter follows:]

DR. MARY ELIZABETH CLUTTER

Birthplace: Charleroi, PA.

Current Address: National Science Foundation, Washington, D.C. 20550 Phone (202) 357-9443.

Current Position: Senior Science Advisor to the Director, National Science Foundation.

Education: Allegheny College, B.S. 1953, Biology; University of Pittsburgh, M.S. 1957, Botany; University of Pittsburgh, Ph.D. 1960, Botany; and Yale University, Postdoctoral, 1960-1962, Biology.

Additional Training: Federal Management Training, Coolfont, West VA, 1976; Executive Leadership and Management, Federal Executive Institute, Charlottesville, VA, 1979; Executive Development, Kings Point, NY, 1980; and Managing People, Wharton School, Philadelphia, 1982.

Employment: Yale University, Biology Department, Research Associate, 1962-73, Lecturer, 1965-78.

National Science Foundation, Program Director, Developmental Biology, 1976-81, Action Section Head, Cellular and Physiological Biosciences, 1981-82, Section Head, Cellular and Physiological Biosciences, 1982-84, Division Director, Cellular Biosciences, 1984-85.

Honors and Awards: Committee on Nominations, elected. American Association for the Advancement of Science, 1979.

Chair, Executive Council, elected. Federal Executive Institute. 1979.

Selected for Senior Executive Service Candidate Development Program. 1980.

Fellow of the AAAS. 1981. Citation: "For services to scientific societies and for extraordinary service to the scientific community as a responsible, effective and imaginative reviewer and administrator of public funds."

Career Appointment to the Senior Executive Service. 1982.

Committee on Nominations, Section G, elected. AAAS. 1983.

Senior Executive Service Awards. 1983, 1984.

Presidential Rank Award. 1985.

Member, Board of Directors, elected. AAAS. 1986.

Professional Societies: American Association for the Advancement of Science; American Society for Cell Biology; American Society of Plant Physiologists; Association for Women in Science; International Society for Plant Molecular Biology; Sigma Xi; Society for Developmental Biology; and Member of the Corporation, Marine Biological Laboratory, Woods Hole.

STATEMENT OF DR. MARY E. CLUTTER, SENIOR SCIENCE ADVISOR, OFFICE OF THE DIRECTOR, NATIONAL SCIENCE FOUNDATION, WASHINGTON, DC

Dr. CLUTTER. Good morning. Thank you very much for inviting me to testify. We at the National Science Foundation are always happy to come over to talk about the process we use for selecting research projects because we're very proud of the process. We think it works very well.

The previous witnesses have talked about the processes used in the mission-oriented agencies. Well, as you know, the National Science Foundation's mission is to support the health of all of science and engineering and the science education enterprise, so we bear a heavy responsibility for the whole of the scientific enterprise in this country, and in the main we support what we call fundamental research or basic research.

Because my testimony is rather lengthy, I have submitted it for the record, and I would like to summarize it and make five major points which are the key points of my testimony.

No. 1, I want to tell you how projects are selected by NSF; No. 2, who makes the selections; No. 3, the safeguards to the system to ensure for an open and fair process; No. 4, I want to talk about the changes that have taken place in the past 10 years, because as you all know, the peer review process is not a static one and it has certainly undergone many changes since it was first invented more than 300 years ago by the Royal Society of London; and last, I would like probably to spend most of my time on the unadvertised benefits of this review process. I think they're very important and I think that we don't often emphasize those benefits. We often hear about the criticisms more often than the positive aspects of peer review.

First of all, I'll go back to the first point. Just how are projects selected by the National Science Foundation? And if you look at the very end of my prepared testimony, you will see a diagram that summarizes the basic process that we use. This is the process known as peer review (chart 1), and I'll just run through this because I think there are a couple of elements that we will want to emphasize.

THE REVIEW PROCESS

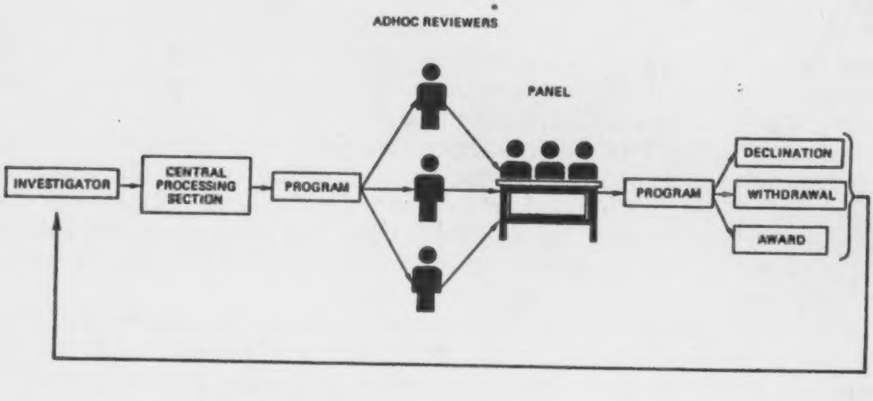


CHART 1

First of all, most of the proposals that the Foundation receives—in fact, I'd say 99.9 percent of them—are unsolicited proposals submitted to the various programs of the Foundation. Of course, the programs are well advertised. They are sent to the National Science Foundation and the proposals are assigned to a program—and I want to emphasize at this point that those proposals are not assigned by a computer or some unknowledeable clerk. Scientists and engineers have a look at those proposals and make sure they are assigned to the right programs. If there is a problem that a proposal, for example, falls between two disciplines, we have program officers who talk to other program officers in other parts of the Foundation and very often will arrange for a joint review, to make sure that things don't fall into the cracks, as we like to say.

All of the proposals are read by our program officers and they are sent out to what we call "ad hoc mail reviewers." This is a very important part of the process, we think, because we send those proposals to experts in the field of the proposal, wherever those experts are. So the expert could be in Japan or England, or in a small college in this country, wherever, or in industry, wherever. We send those proposals to the experts and solicit their opinions.

If you will look, I think, on page 7 of my testimony, you will see the criteria that we ask the reviewers to use in evaluating the proposal. We ask them to look particularly at the research performance competences, the competence of the principal investigator. And so that means looking at the track record of the person who is proposing to do the research. We ask them to comment on the intrinsic merit of the research. We ask them to talk about the utility or relevance of the research—and that third criterion we especially ask for when the research is of an applied nature. And last, we ask them to comment on the effect on the infrastructure of science and engineering; that is, what the work will contribute to the Nation's research, education, and human resource base.

Now, I've added for the record the form that we send out, the actual review form, so you can have a look at that to see exactly what we ask of our mail reviewers.

After we receive the mail reviews, we follow a couple of procedures. On the diagram it shows that we convene a panel. Well, this is true in certain disciplines, where it's the customary procedure to have a panel convened. I happen to come from the biological sciences, and in the biological sciences we feel very comfortable with that procedure. So we ask people who represent all of the areas of a particular discipline—and in my own, that's in developmental biology—to come sit with us to discuss all the proposals and the ad hoc reviews and give us advice on what projects we should select for support.

In other programs at the Foundation, especially ones that are reviewing very large projects, we will have a site visit team go out and visit every project that's submitted and make an evaluation of the project and the institution's capability for handling the project.

When all of these reviews are finished, our program officers sit down and decide what projects they are going to recommend for support, depending on a lot of factors, including the amount of money that's available to support the projects.

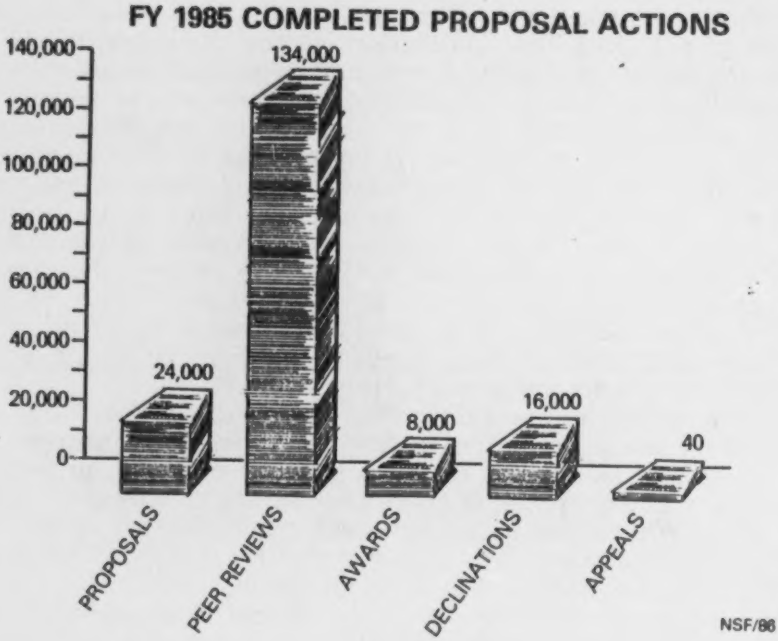


CHART 2

If you will look at the next diagram, you will see the scope of our operations (chart 2). We have some data from fiscal year 1985 and you see that we receive something like 24,000 proposals in the Foundation. We solicited 134,000 reviews of those proposals. We made 8,000 awards and, of course, we were forced to make 16,000 declinations, even though many of those projects were certainly worthy of support. And then finally, in the last stack there, the very small one, you'll see that we did have 40 appeals. And we don't have the final stack, which was the appeals that resulted in a decision in favor of the principal investigator—we had five such cases. So we feel the appeals process works.

Well, that's the basic system that we use for selection of research projects.

My second point is, just who makes the selections? Well, we utilize that vast community out there of peer reviewers, of expert scientists, to help give us advice. But ultimately the decision is made by people like me. I came to the Foundation from a university 9 years ago as a rotator. I am a scientist. I have been involved in the peer review process from all sides. I've submitted proposals, I've received awards, I've had some proposals declined. I've been a program officer at the Foundation. I've been a division director, and now I give advise to the Director of NSF.

I came to the Foundation as a rotator, and we have about 300 program officers in the Foundation and a third of them are rotators. I want to correct the written testimony. I think that we said about 70 out of 300 are rotators. The actual number is 100 out of

300. And we like the system because what it does is to bring new ideas every year into the Foundation, so the same people are not making the decisions over and over and over. And people like me, who started as rotators—and I think that most of our people who are now permanent started as rotators in the Foundation—move out of the programs, so that we're not making that scientific decision over and over the years, but move out and move up and monitor the system, as it were, and make a higher level review decision.

For some of our larger projects that involve fairly sizeable amounts of money the decisions are—or the recommendations of the program officers are referred to a higher level. The assistant directors of the directorates of the Foundation review the recommendations and, in some cases, the National Science Board reviews the recommendations and recommends support.

My third point is the safeguards to ensure a fair and open system. We have a very firm policy on conflicts of interest, and these are addressed on that review form that we send out to every reviewer of every proposal. If there's a conflict of interest of a program officer with a particular proposal, that program officer does not review that proposal, so we have very firm and established policies regarding the conflict-of-interest situation.

Second, we have an appeals process, which I showed you on the diagram, so that if a principal investigator is unhappy with the decision, that person can appeal—and we have a process in place.

Third, we have audit and oversight within the foundation, an office that regularly samples the awards and declinations of all of the programs of the Foundation to see whether the process was a fair one in making the final decision.

Fourth, we have external oversight review so that every program in the Foundation is reviewed by an external, a visiting committee, if you will, once every 3 years. And finally, we have a program evaluation staff that does fairness studies.

My fourth major point concerns changes that have occurred during the past 10 years. Is the peer review process static? No, it's a dynamic process. And so I would like to go over briefly the changes that have occurred, first of all to the review process itself, and finally, to broaden the participation of scientists in this process.

In the review process, ever since 1976, we have sent to all principal investigators verbatim anonymous reviews. That means that all those mail reviews that come in for a given proposal are sent verbatim without the reviewer's name, of course, to the principal investigator. Now, we don't send them to anyone else, and I think we want to emphasize that. We don't send the reviews to the institution; we don't send them to the newspapers; we only send them to the person who proposed the work.

Second—and I mentioned this before—we do have an appeal procedure now, and that's new within the last 10 years. Third, the Office of Audit and Oversight was established within the last 10 years. Also, the external program oversight review committees. We want to reduce the paperwork so we have limited the page numbers on proposals, and so, since 1978, the limitation on proposals is 15 pages. There are a couple of exceptions to that, which I'll bring out later.

The other change in the review process is that we now make creativity extensions, so that if during the course of a project special creativity has been demonstrated, a project officer, without further external peer review, can recommend for internal review a 2-year extension without a new proposal.

The seventh new thing is that we now have accomplishment-based renewals, and these are a variation on that creativity extension, except a principal investigator submits not a full blown proposal, not the 15-page variety, but a 4-page proposal, plus the papers that that person has published over the last 5 years. And those are reviewed and awards, hopefully, come from that.

And finally, we now have the Presidential Young Investigator Program. That program involves no proposal at all, and we make 200 of these awards each year to very promising young investigators. They are 5-year awards and we make those awards to people who have been nominated by their institutions as having outstanding promise in their field.

Now, I've said also that during the past 10 years we've made efforts to broaden participation of the scientific community in the research enterprise, and I will mention a few of the programs. First of all, we're very proud of what we call the EPSCORE Program. This is the experimental program to stimulate competitive research in some of the States that have not been very competitive for Federal funding.

Second, we have a special program that serves predominantly undergraduate institutions, and we're happy to say that since the beginning of that program in 1983 we have increased support to those institutions by almost 68 percent.

We also have a program that we call ROA, that's a research opportunity award, which does not require a full-blown proposal. With a one- or two-page letter, an investigator from a predominantly undergraduate institution can ask for support to work with a person who has been peer reviewed, who has a research project supported by NSF at another institution. And so, by making a fairly simple supplement, if we have the funds, we can support that person to work with somebody who is more established.

We also have instituted in several of our directorates what we call minority supplements, in which we can add enough money to pay for a minority student, who is an undergraduate or a graduate student—in some cases postdocs or in some cases high school students—to work with established investigators on their research grants for a very minor investment.

We have a new program which we call a retooling supplement program in several of our divisions in the Foundation. Because, as you all know, if you haven't been very close to some of the developments, especially in areas like biotechnology, where there are some new techniques that have been developed over the last 5 or 6 years, if you happen to get your Ph.D. say in 1974, you could be out of it as far as some of these techniques are concerned. We're worried about some of those people who may be 40 years old who still have another 20 years of good research left, probably, and they don't really have the tools necessary, or don't know the techniques. So we can make a supplement to that person so that they can go to the place that they need to go to be retooled. That's also an expensive program, but we hope a very effective one.

We have a new program in the engineering directorate for novel research ideas, and we can make awards within a 1 month's turnaround period, very small awards, but for new ideas, high-risk ideas, and we do that without external peer review. But I want to emphasize that we have set aside just a small amount of money for that and we're doing it on a 1-year experimental basis, to see how well that works out. What happens is an investigator presents a novel idea to a program officer in the engineering directorate, where we're trying it out, and that idea is reviewed internally. Then we can make an award for up to $30,000 to see whether that idea can pay off.

And finally, in trying to increase the participation, we want to increase the number of people from small colleges, scientists from small colleges, on our review committees. And so Erich Bloch, the Director of NSF, recently sent out a letter—and we've added that for the record—asking presidents of these colleges to help us make it possible for scientists from their institutions to sit on our committees.

Last, I want to talk about those unadvertised benefits that you don't often hear about of the peer review process.

First of all, there's the educational value. You know, when you send proposals out for peer review, the mail review, you send them out to experts wherever they are. Well, some of the people are clearly people who are in States that are perhaps not really closely connected, or institutions that are not closely connected with the centers of research. And if we send out a proposal with state-of-the-art research—and we hope that's the case—to that person, it helps them. It broadens them without the expenditure of any money.

The same goes in the international arena. It really helps to send proposals on both sides—it works both ways—to people, say, in France or Germany or England, wherever. And we all benefit from that.

The second thing is that it results in better research, because, you know, you hear people say, "You get these unwarranted criticisms in some of these reviews." Well, that's true. Sometimes they're anonymous and sometimes people make statements they probably wouldn't make to a person's face. But my experience is that, for the most part, people who review proposals do it very seriously. They get caught up in the excitement of the research, and they might think of another experiment, and so they'll suggest that experiment in the review, which helps make that a better research project. I can expend on this, but I won't, since the time is short.

Third, it increases the visibility of the proposer. By exposing that person to this vast community of reviewers, that person gets to be known. And I have known cases where people who were relatively unknown in their field came to the attention of people who were setting up symposia or meetings and they have invited that person to be a speaker. So it has increased that person's visibility.

Fourth—I think this is really important—there is the leveraging aspect of the peer review process, because as we said at the outset, we really aren't in a position to fund all of the worthy projects that come in to us. We fund about one out of four proposals. And that rate may well be going down because of the budget climate. However, a favorable review can do a lot for a project because a PI is often in a position then to obtain matching funds. And I would just like to give you a couple of examples of cases where people have leveraged those dollars.

For example, in the Research Opportunities for Women Program, there was an investment of $156,000 which was leveraged, and so that the total investment became $1½ million. In the case of the EPSCORE program—and that's the experimental program to stimulate competitive research in States—NSF has invested $36 million in that program and States and other non-Federal sources have matched that, or more than matched it, with an investment of $113 million.

Another example, our supercomputer centers—and I'll give you the example of the Pittsburgh center. NSF is going to invest $36 million over a 5-year period. We now have commitments from the State and other non-Federal sources of $72.9 million added to the $36 million over a period of 5 years. So that's another example of leveraging.

In the Engineering Research Centers, the Delaware center. NSF invested $1¼ million in fiscal year 1986, and the State and other sources have invested $1½ million. So the leveraging is not a trivial matter in this peer review process.

And finally, I would like to say that in the international arena our peer review process in the United States is considered to be a great strength of the system. It's something that other countries don't have. And the People's Republic of China recently came to the National Science Foundation and asked us to help them set up a comparable peer review system in China.

With that, I will conclude my lengthy remarks, which were supposed to be a summary. Thank you.

[The prepared statement of Dr. Clutter follows:]

STATEMENT OF
DR. MARY E. CLUTTER
SENIOR SCIENCE ADVISER
OFFICE OF THE DIRECTOR
NATIONAL SCIENCE FOUNDATION

HEARINGS ON RESEARCH PROJECT SELECTION
BEFORE THE
TASK FORCE ON SCIENCE POLICY
COMMITTEE ON SCIENCE AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

WASHINGTON, D.C.
WEDNESDAY, APRIL 9, 1986

THANK YOU, MR. CHAIRMAN, FOR INVITING ME TO TESTIFY ON HOW NSF REVIEWS AND SELECTS RESEARCH PROPOSALS. THIS SUBJECT HAS BEEN OF GREAT INTEREST TO ME FOR MANY YEARS -- FIRST AS AN APPLICANT, AN INVESTIGATOR AND REVIEWER, THEN AS AN NSF PROGRAM OFFICER AND EVENTUALLY AS DIRECTOR OF A RESEARCH SUPPORT DIVISION HAVING RESPONSIBILITY FOR SEVERAL PROGRAMS. IN MY PRESENT POSITION AS SENIOR SCIENCE ADVISER TO THE DIRECTOR, I HAVE HAD ADDITIONAL OPPORTUNITIES TO OBSERVE AND REFLECT ON THE PROPOSAL SELECTION PROCESS ACROSS THE ENTIRE FOUNDATION.

I WOULD LIKE TO BEGIN BY DESCRIBING THE FOUNDATION'S OVERALL SYSTEM FOR HANDLING A PROPOSAL FROM THE TIME WE RECEIVE IT THROUGH THE TIME WHEN AN INSTITUTION IS FORMALLY NOTIFIED OF AN AWARD OR DECLINATION. THEN I WILL DESCRIBE IN MORE DETAIL THE INVOLVEMENT OF EXTERNAL REVIEWERS, PARTICULARLY HOW THEY ARE SELECTED AND WHAT WE ASK THEM TO DO.

I WILL COMMENT ON THE RESPONSIBILITIES AND SCOPE OF THE PROGRAM OFFICER IN MAKING RECOMMENDATIONS AND POINT OUT THE MEASURES WE HAVE TAKEN OVER THE PAST FEW YEARS TO INSURE THAT THE SYSTEM IS AS OPEN AND FAIR AS POSSIBLE. FINALLY, I WILL MENTION SEVERAL RECENT EFFORTS TO REDUCE THE TIME FOR PROPOSAL DECISIONS, FURTHER REDUCE THE BURDEN ON THE APPLICANT, INVITE GREATER PARTICIPATION IN THE REVIEW PROCESS, AND GIVE SPECIAL ENCOURAGEMENT AND CONSIDERATION TO ESPECIALLY NOVEL AND HIGH-RISK RESEARCH.

THAT IS A LOT OF MATERIAL TO COVER, SO I WILL ADDRESS THESE TOPICS BRIEFLY AND IN GENERAL TERMS TO ALLOW SUFFICIENT TIME FOR YOUR QUESTIONS. I AM ALSO PREPARED TO SUBMIT VARIOUS DOCUMENTS FOR THE RECORD. WHILE MY COMMENTS ARE CENTERED ON RESEARCH PROJECT SELECTION, MOST OF WHAT I HAVE TO SAY APPLIES AS WELL TO PROPOSALS IN OUR OTHER PROGRAMS SUCH AS SCIENCE AND ENGINEERING EDUCATION.

EXCEPT FOR A FEW NOTABLE LARGE-SCALE INITIATIVES SUCH AS THE ENGINEERING RESEARCH CENTERS AND SUPERCOMPUTER CENTERS, MOST PROPOSALS RECEIVED BY THE FOUNDATION ARE NOT IN RESPONSE TO

SPECIFIC SOLICITATIONS OR FORMAL REQUESTS-FOR-PROPOSALS OF THE TYPE THAT MANY OTHER AGENCIES EMPLOY. WE DO, OF COURSE, PUBLICIZE OUR PROGRAMS EXTENSIVELY, SPECIFY THE FORMAT FOR PROPOSALS, AND ENCOURAGE DIRECT CONTACTS WITH OUR PROGRAM STAFF. BUT THE BULK OF THE PROPOSALS ARE UNSOLICITED; THAT IS, THE RESEARCH IDEAS AND METHODS ARE THOSE OF THE APPLICANTS.

THE MECHANICS OF PROPOSAL HANDLING CAN BE DESCRIBED SIMPLY. THE RESEARCH PROGRAMS OF THE FOUNDATION ARE ORGANIZED ALONG THE LINES OF FIELDS OF SCIENCE AND ENGINEERING. IN THE FY 1987 BUDGET YOU WILL FIND SOME 140 RESEARCH SUPPORT PROGRAMS IN 27 DIVISIONS. THE PROPOSALS COME INTO A CENTRAL UNIT, WHERE A FILE IS . ESTABLISHED AND THEY ARE ASSIGNED TO THE APPRUPRIATE DIVISION FOR HANDLING. THEY ARE THEN FURTHER ASSIGNED TO A PROGRAM OFFICER. IT IS IMPORTANT TO NOTE THAT THESE PROGRAM ASSIGNMENTS ARE MADE BY SCIENTISTS AND ENGINEERS WHO KNOW THE SUBJECT MATTER.

BEFORE I TELL YOU ABOUT THEIR IMPORTANT ROLE, LET ME DESCRIBE A TYPICAL PROGRAM OFFICER. COUNTING DIVISION AND SECTION CHIEFS, THERE ARE ABOUT 300, OR MORE THAN A QUARTER OF THE TOTAL FOUNDATION STAFF. ALL OF THEM HAVE SIGNIFICANT RESEARCH EXPERIENCE, AND ALMOST ALL HAVE DOCTORATES. THEY ARE RECRUITED FOR THE BREADTH AND DEPTH OF THEIR SPECIALIZED KNOWLEDGE AS WELL AS FOR THE PERSONAL QUALITIES THEY WILL NEED TO DEAL WITH THE PROMINENT SCIENTISTS IN THEIR FIELD AND THE MANY OTHER DEMANDS OF THE POSITION.

AT ANY ONE TIME, ABOUT A THIRD ARE ONE- AND TWO-YEAR "ROTATORS" ON LEAVE FROM THEIR UNIVERSITIES AND COLLEGES. THE FOUNDATION PLACES A VERY HIGH VALUE ON RECRUITING AND USING "ROTATORS" BECAUSE THEY HAVE RECENT BENCH-LEVEL EXPERIENCE AND ARE ABLE TO INFUSE OUR DECISION-MAKING SYSTEMS WITH THEIR FRESH VIEWPOINTS. MANY OF THE PERMANENT STAFF BEGAN AS "ROTATORS". IN MY OWN CASE, FOR EXAMPLE, I WAS AN ESTABLISHED INVESTIGATOR AT YALE WHO INITIALLY AGREED TO COME WITH THE FOUNDATION FOR TWO YEARS -- AT THE END OF WHICH TIME I WAS OFFERED A PERMANENT POSITION AND HAD TO MAKE THE DIFFICULT DECISION WHETHER TO GIVE UP MY OWN RESEARCH IN THE INTERESTS OF SERVING THE GOVERNMENT AND MY FIELD OF SCIENCE.

OVER THE COURSE OF SEVERAL MONTHS, THE PROGRAM OFFICER OVERSEES THE EXTERNAL REVIEW, EVALUATES THE COMMENTS OF THE REVIEWERS, AND MAKES A RECOMMENDATION AS TO WHETHER THE PROPOSAL SHOULD BE AWARDED OR DECLINED, TAKING INTO ACCOUNT OTHER CONSIDERATIONS -- SUCH AS THE RELATIONSHIP OF A PARTICULAR PROPOSAL TO THE FIELD AS A WHOLE AND TO OTHER PENDING PROPOSALS, AND THE AMOUNT AVAILABLE IN THE PROGRAM'S BUDGET.

THE PROGRAM OFFICER'S RECOMMENDATIONS ARE REVIEWED AT ONE OR MORE HIGHER LEVELS. APPROVAL RECOMMENDATIONS ARE THEN FORWARDED TO SPECIALISTS IN THE DIVISION OF GRANTS AND CONTRACTS, WHO CHECK ON ALL THE NONSCIENTIFIC ASPECTS OF THE AWARD AND ISSUE THE FORMAL NOTIFICATION TO THE APPLICANT'S INSTITUTION. IN SOME CASES, AS WITH A SIZABLE REQUEST FOR FUNDS OR, FOR EXAMPLE, THE FIRST GROUP

OF PROPOSALS IN A NEW PROGRAM, THE RECOMMENDATIONS ARE FURTHER REVIEWED BY AN ASSISTANT DIRECTOR AND BY THE NATIONAL SCIENCE BOARD.

ENSURING THE QUALITY, COMPLETENESS AND FAIRNESS OF THE EXTERNAL REVIEW IS ONE OF THE PRINCIPAL RESPONSIBILITIES OF THE PROGRAM OFFICER. THE REVIEW PROCESS EMPLOYED DEPENDS ON THE TRADITION OF THE PARTICULAR FIELD OF RESEARCH: IN THE PHYSICAL SCIENCES AND ENGINEERING, PROPOSALS ARE USUALLY MAILED TO REVIEWERS WHO RESPOND INDIVIDUALLY; IN THE BIOLOGICAL, BEHAVIORAL AND SOCIAL SCIENCES, A PANEL OF SCIENTISTS REVIEWS THE PROPOSAL IN ADDITION TO THE MAIL REVIEW. FOR LARGE PROJECTS AND SPECIAL COMPETITIONS OF VARIOUS TYPES, REVIEWS ARE OFTEN SUPPLEMENTED WITH SITE VISITS BY NSF STAFF AND TEAMS OF OUTSIDE EXPERTS.

IN THE MAIL-OUT PROCEDURE, THE PROGRAM DIRECTOR SENDS EACH PROPOSAL TO SEVERAL PEOPLE -- SOMETIMES AS MANY AS TEN -- WHOM HE OR SHE HAS IDENTIFIED AS KNOWLEDGEABLE ON THE TOPIC. THE REVIEWER RECEIVES STANDARD INSTRUCTIONS AND REVIEW FORMS, AND RESPONDS DIRECTLY TO THE PROGRAM OFFICER. WHERE PANELS ARE USED, GROUPS OF EXPERTS MEET AT SOME SPECIFIED INTERVAL -- GENERALLY THREE TIMES A YEAR IN THE LARGER PROGRAMS -- TO WEIGH A GROUP OF PROPOSALS, TAKING INTO ACCOUNT THE COMMENTS OF THE AD HOC REVIEWERS. THE FIRST CRITERION IN CHOOSING PANELISTS IS EXPERTISE IN THE FIELD; THEN WE LOOK TO BALANCE THE PANELS WITH REGARD TO SUCH FACTORS AS INSTITUTIONAL SIZE AND PARTICIPATION OF WOMEN AND MINORITIES. PANELISTS SERVE FOR A SPECIFIED PERIOD OF TIME;

COMMONLY, SOME PORTION OF THE PANEL WILL ROTATE OFF EACH YEAR.

BOTH METHODS OF PEER REVIEW HAVE PROS AND CONS, AND BOTH HAVE ADVOCATES WHO STRONGLY DEFEND THEM AND WOULD BE VERY RELUCTANT TO CHANGE. THE TYPE OF REVIEW USED IN A PROGRAM HAS GROWN OUT OF THE SCIENTIFIC TRADITION IN THAT PARTICULAR FIELD; MOST APPLICANTS IN A GIVEN FIELD ARE FAMILIAR WITH IT AND UNDERSTAND IT. ON AVERAGE, NO MATTER WHICH METHOD IS USED, A PROPOSAL HAS THE BENEFIT OF 5.5 COMPLETED EXTERNAL REVIEWS.

TO GIVE YOU SOME IDEA OF THE SCALE OF OPERATIONS, DURING FY 1985 THE FOUNDATION TOOK FINAL ACTIONS ON 24,403 COMPETITIVELY REVIEWED PROPOSALS. OF THESE, 7,968 WERE AWARDED; ALMOST TWICE AS MANY, 15,504, WERE DECLINED; AND 931 WERE WITHDRAWN BY APPLICANTS PRIOR TO A DECISION. OBVIOUSLY THEN, OVERALL, ABOUT ONE-THIRD OF THE TOTAL COMPLETED ACTIONS BECAME AWARDS. SOME PROGRAMS HISTORICALLY HAVE HIGHER RATIOS OF AWARDS TO DECLINES, SOME LOWER, DEPENDING ON A LOT OF FACTORS INCLUDING THE SIZE OF THE PROGRAM BUDGET AND THE NATURE OF A TYPICAL AWARD IN THAT FIELD.

IN FISCAL 1985 A TOTAL OF 59,725 INDIVIDUAL REVIEWERS AND PANELISTS CONTRIBUTED THEIR SERVICES TO NSF. THEY CAME FROM A WIDE RANGE OF INSTITUTIONS -- NOT ONLY ACADEMIC INSITUTIONS, LARGE AND SMALL, BUT FROM INDUSTRY, GOVERNMENT AND EVEN FOREIGN RESEARCH AND EDUCATIONAL ORGANIZATIONS.

AS THAT TOTAL SHOWS, PROGRAM OFFICERS ARE ALWAYS ON THE LOOKOUT

FOR QUALIFIED, KNOWLEDGEABLE REVIEWERS FROM ALL BACKGROUNDS. THERE ARE VERY PRACTICAL REASONS FOR THAT: FRESH INSIGHTS AND ADVICE CAN BE A VALUABLE CONTRIBUTION TO DECISIONS, PARTICULARLY WHEN ONE IS TRYING TO DISCERN THE BEST PROPOSAL AMONG MANY OF SUBSTANTIALLY EQUAL MERIT; AND CERTAINLY ONE DOES NOT WANT TO OVERLOAD INDIVIDUAL REVIEWERS (HOWEVER KNOWLEDGEABLE) WITH TOO MANY REQUESTS.

THE SPECIFIC FACTORS THAT WE ASK REVIEWERS TO TAKE INTO CONSIDERATION ARE SPELLED OUT FOR APPLICANTS IN OUR PUBLICATION ENTITLED "GRANTS FOR SCIENTIFIC AND ENGINEERING RESEARCH". BASICALLY, THE NATIONAL SCIENCE BOARD HAS SET FORTH FOUR CRITERIA TO BE APPLIED TO ALL PROPOSALS "IN A BALANCED AND JUDICIOUS MANNER". THEY ARE:

(1) RESEARCH PERFORMANCE COMPETENCE: THAT IS, THE TECHNICAL SOUNDNESS, THE CAPABILITY OF THE INVESTIGATOR(S), AND THE ADEQUACY OF INSTITUTIONAL RESOURCES AVAILABLE FOR THE WORK;

(2) THE INTRINSIC MERIT OF THE RESEARCH: THAT IS, THE EXTENT TO WHICH THE PROPOSED WORK IS EXPECTED TO LEAD TO NEW DISCOVERIES OR FUNDAMENTAL ADVANCES IN ITS FIELD OR ACROSS FIELDS OF RESEARCH;

(3) UTILITY OR RELEVANCE: THE EXTENT TO WHICH THE WORK COULD CONTRIBUTE TO AN EXTRINSIC GOAL SUCH AS A NEW TECHNOLOGY; AND

(4) THE EFFECT ON THE INFRASTRUCTURE OF SCIENCE AND ENGINEERING; THAT IS, WHAT THE WORK WILL CONTRIBUTE TO THE NATION'S RESEARCH, EDUCATION AND HUMAN RESOURCE BASE.

THE RELATIVE WEIGHTS OF CRITERIA (2) AND (3), INTRINSIC MERIT AND UTILITY, DEPEND ON THE CHARACTER OF THE WORK. THE FOURTH CRITERION ALLOWS THE PROGRAM TO TAKE INTO ACCOUNT SUCH QUESTIONS AS THE PARTICIPATION OF WOMEN AND MINORITIES, INSTITUTIONAL DISTRIBUTION, AND THE STIMULATION OF IMPORTANT BUT UNDERDEVELOPED FIELDS OF RESEARCH. IN MANY CASES PROGRAMS ALSO USE SPECIAL CRITERIA RELATED TO THEIR PURPOSE AS IN, FOR EXAMPLE, SPECIALIZED PROGRAMS FOR EQUIPMENT GRANTS OR VISITING PROFESSSORSHIPS FOR WOMEN. IN ANY EVENT, THE SCIENTIFIC MERIT OF THE PROPOSED WORK AND THE COMPETENCE OF THE APPLICANT ARE THE PRINCIPAL FACTORS.

REVIEWERS ARE ASKED TO PROVIDE NOT JUST AN OVERALL RATING BUT NARRATIVE COMMENTS, INCLUDING COMMENTS ON THE RESEARCHER'S EARLIER COMPLETED WORK WHERE PERTINENT. THESE NARRATIVES ARE MORE VALUABLE THAN THE RATINGS THEMSELVES, NOT JUST TO PROGRAM OFFICERS IN ARRIVING AT THEIR RECOMMENDATIONS BUT TO APPLICANTS, WHO ROUTINELY RECIEVE THEM, AS I WILL EXPLAIN IN A MOMENT.

THE PROGRAM OFFICER'S JOB IS ONLY HALF FINISHED ONCE THE REVIEWS HAVE BEEN COMPLETED. IT IS EASY TO MAKE RECOMMENDATIONS TO FUND VERY HIGHLY RATED PROPOSALS AND TO DECLINE VERY LOW RATED ONES. BUT THERE IS ALWAYS A MIDDLE GROUP, SOMETIMES A SIZABLE ONE, OF MERITORIOUS PROPOSALS ALL OF WHICH WOULD BE WORTH FUNDING IF

ENOUGH MONEY WERE AVAILABLE. BECAUSE THAT'S RARELY THE CASE, WE CALL ON THE PROGRAM OFFICER TO USE HIS OR HER SCIENTIFIC JUDGEMENT, EXPERIENCE AND DISCRETION IN SELECTING AMONG THEM.

THE FOUNDATION HAS A TRADITION OF SELECTING PROGRAM OFFICERS NOT ONLY FOR THEIR RESEARCH EXPERIENCE AND OTHER TECHNICAL QUALIFICATIONS BUT FOR THEIR INTEGRITY AND OBJECTIVITY. AND AS I INDICATED, THEIR RECOMMENDATIONS ARE EXAMINED IN TURN BY MORE EXPERIENCED SECTION AND DIVISION DIRECTORS FAMILIAR WITH THE BROADER SCIENTIFIC AND POLICY ENVIRONMENT WITHIN WHICH THE FOUNDATION OPERATES.

TO ENSURE AN OPEN AND FAIR SYSTEM, WE HAVE BUILT IN SEVERAL OTHER SAFEGUARDS, BOTH FOR THE INDIVIDUAL APPLICANT AND TO SATISFY OURSELVES THAT THE SYSTEM AS A WHOLE IS OPERATING AS IT SHOULD.

FIRST, FOUNDATION POLICIES AND PRACTICES ABOUT POTENTIAL CONFLICTS OF INTEREST, OR EVEN THE APPEARANCE OF CONFLICTS, IS VERY CLEAR AND MADE KNOWN TO REVIEWERS AND PROGRAM OFFICERS. WE ASK THEM TO IDENTIFY ANY ACADEMIC AFFILIATIONS, PERSONAL RELATIONSHIPS OR FINANCIAL INTERESTS THAT COULD CONSTITUTE OR BE REGARDED AS A CONFLICT, AND TO WITHDRAW FROM THE DECISION PROCESS IF WARRANTED.

SECOND, SINCE THE MID-1970'S WE HAVE ROUTINELY SENT ALL OF THE REVIEWS TO THE APPLICANT VERBATIM BUT WITHOUT ATTRIBUTION TO INDIVIDUAL REVIEWERS. THIS SYSTEM PROVIDES FOR THE INTERESTS OF

BOTH APPLICANTS AND REVIEWERS -- REVIEWERS BECAUSE SOME OF THEM MIGHT BE RELUCTANT TO BE ENTIRELY CANDID OTHERWISE; AND APPLICANTS BECAUSE THEY FIND THE REVIEWERS' DETAILED COMMENTS VERY VALUABLE FOR EXPLAINING A DECISION AND FOR REFORMULATING THEIR PROPOSAL IF THEY WISH TO RESUBMIT IT.

THIRD, WE HAVE A SYSTEM FOR FORMAL RECONSIDERATION OF PROPOSAL DECISIONS, AT THE REQUEST OF THE APPLICANT. THIS IS NOT A "DE NOVO" REVIEW OF THE SCIENTIFIC MERITS BUT AN EXAMINATION OF HOW THE CASE WAS HANDLED BY THE PROGRAM OFFICER. RECONSIDERATIONS ARE CARRIED OUT BY PERSONS NOT INVOLVED IN THE EARLIER DECISION. AN APPLICANT NOT SATISFIED WITH THE INITIAL RECONSIDERATION MAY HAVE HIS OR HER INSTITUTION REQUEST A FURTHER RECONSIDERATION BY THE FOUNDATION'S DEPUTY DIRECTOR.

WE ALSO TAKE MEASURES TO EXAMINE OUR PROCESS SYSTEMATICALLY: FIRST, STAFF OF THE DIRECTOR'S OFFICE OF AUDIT AND OVERSIGHT ROUTINELY EXAMINE A SMALL RANDOM SAMPLE OF ACTIONS FOR COMPLIANCE WITH FOUNDATION PROCEDURES AND PROVIDE DIRECT FEEDBACK TO THE PROGRAM DIVISION.

SECOND, EVERY ONE OF THE PROGRAMS IS GIVEN A COMPREHENSIVE REVIEW EVERY THREE YEARS BY A SMALL GROUP OF EXTERNAL PEERS FAMILIAR WITH THE FIELD. THESE "VISITING COMMITTEES", IF YOU WILL, MAKE FORMAL REPORTS TO THE NSF DIRECTOR ABOUT EACH PROGRAM. THE REPORTS, AND THE ANNUAL SUMMARY, ARE PUBLICALLY AVAILABLE.

THESE PEER OVERSIGHT COMMITTEES SELECT THEIR OWN SAMPLE OF ACTIONS AND ADDRESS SUCH MATTERS AS WHETHER THE SELECTION OF REVIEWERS, THE PROGRAM OFFICER'S RATIONALE FOR RECOMMENDING AN AWARD OR DECLINATION, AND THE BALANCE AMONG PROJECTS SELECTED ARE REASONABLE. WE HAVE CONDUCTED THESE REVIEWS FOR SEVERAL YEARS AND I AM PROUD TO SAY THAT THE COMMENTS OF THESE EXTERNAL PEERS ABOUT THE PROFESSIONAL STEWARDSHIP OF THE NSF STAFF ARE GENERALLY VERY FAVORABLE.

THIRD, WE HAVE UNDERTAKEN SOME EXPERIMENTAL STUDIES OF THE PROPOSAL REVIEW AND AWARD SYSTEM AS A WHOLE. OUR PROGRAM EVALUATION STAFF, WHICH IS PART OF THE CONTROLLER'S OFFICE AND THUS INDEPENDENT OF THE PROGRAM DIVISIONS, HAS RUN VARIOUS STATISTICAL TESTS ON CAREFULLY-CONSTRUCTED SAMPLES TO SEE IF THE SYSTEM HAS BEEN "FAIR" TO CERTAIN CLASSES OF APPLICANTS. HERE ARE SOME OF THE FINDINGS:

(1) FOR NSF AS A WHOLE, ABOUT ONE OF EVERY FOUR NEW PROPOSALS ARE FUNDED, AND ABOUT ONE OF TWO RENEWAL PROPOSALS; GENERALLY, RENEWAL PROPOSALS RECEIVE BETTER RATINGS, BUT EVEN WHERE THE RATINGS ARE ABOUT THE SAME, THE RENEWAL IS SOMEWHAT MORE LIKELY TO BE FUNDED.

(2) PROPOSALS OF ROUGHLY THE SAME QUALITY AS INDICATED BY THE RATINGS HAVE THE SAME CHANCE OF BEING AWARDED OR DECLINED NO MATTER WHAT STATE THE PROPOSALS ARE FROM.

(3) BEYOND THE RATINGS, AND WHETHER THE PROPOSAL IS NEW OR A RENEWAL, NONE OF THE FOLLOWING THREE FACTORS HAVE A STATISTICALLY SIGNIFICANT EFFECT ON DECISIONS: THE PRESTIGE OF THE APPLICANT'S DEPARTMENT, HIS OR HER "PROFESSIONAL AGE", OR THE AMOUNT OF MONEY REQUESTED.

(4) THERE ARE OTHER LESS GLOBAL BUT NEVERTHELESS INTERESTING RESULTS. FOR EXAMPLE, THERE IS NO STATISTICALLY SIGNIFICANT DIFFERENCE IN THE RATINGS THAT MALE REVIEWERS GIVE TO PROPOSALS FROM WOMEN COMPARED TO MEN.

IN THE FINAL PORTION OF MY STATEMENT, I WOULD LIKE TO ADDRESS TWO FREQUENTLY-HEARD CRITICISMS OF THE FOUNDATION'S REVIEW PROCESS: FIRST, THAT PROPOSALS FROM LESS PROMINENT OR SMALLER RESEARCH INSTITUTIONS ARE NOT WELCOME OR ARE NOT FAIRLY TREATED; AND SECOND, THAT THE SYSTEM IS TOO "BUREAUCRATIC", BURDENSOME TO THE APPLICANT AND NOT FLEXIBLE ENOUGH TO SUPPORT INNOVATIVE AND UNUSUAL PROPOSALS.

THE FOUNDATION HAS MOUNTED A SPECIAL EFFORT -- NOW IN ITS THIRD YEAR -- TO STIMULATE PROPOSALS FROM PLACES OTHER THAN THE LARGE DOCTORAL-GRANTING UNIVERSITIES. THIS EFFORT HAS SUCCEEDED IN INCREASING THE NUMBER OF PROPOSALS AND AWARDS AND THE AMOUNT OF NSF RESEARCH FUNDS TO SMALLER INSTITUTIONS. THERE ARE SEVERAL STRATEGIES INVOLVED: AN OUTREACH PROGRAM TO TAKE INFORMATION ABOUT OUR PROGRAMS, POLICIES AND PROCESSES TO THEIR CAMPUSES; A SYSTEM OF INTERNAL FUNDING TARGETS; AND SPECIAL INSTRUCTIONS

ASKING REVIEWERS TO TAKE INTO ACCOUNT THE CIRCUMSTANCES OF APPLICANTS AT THESE INSTITUTIONS -- FOR EXAMPLE, THE TENDENCY TO PUBLISH LESS FREQUENTLY THAN THEIR COLLEAGUES AT LARGE GRADUATE UNIVERSITIES.

A FOURTH STRATEGY IS A PROGRAM KNOWN AS "RESEARCH OPPORTUNITY AWARDS"; THAT IS, A MECHANISM WHEREBY THE PROGRAM OFFICER CAN ADD A MODEST AMOUNT OF FUNDS TO EXISTING GRANTS OR PENDING PROPOSALS TO COVER PARTICIPATION BY RESEARCHERS FROM SMALL INSTITUTIONS. THIS CAN BE DONE WITH OR WITHOUT EXTERNAL REVIEW ON THE BASIS OF A LETTER OR OTHER BRIEF APPLICATION.

WE HAVE ALSO TAKEN STEPS TO INCREASE THE PARTICIPATION OF PERSONS FROM PREDOMINANTLY UNDERGRADUATE INSTITUTIONS IN THE MEMBERSHIP OF OUR ADVISORY COMMITTEES AND REVIEW PANELS. LAST MONTH THE DIRECTOR SENT A LETTER TO THE PRESIDENTS OF THESE COLLEGES AND UNIVERSITIES ENLISTING THEIR HELP IN THIS EFFORT.

WE HAD FOUND THAT SOME FACULTY MEMBERS AT THESE INSTITUTIONS ARE RELUCTANT TO SERVE, PARTICULARLY ON PANELS, FOR THREE REASONS: REVIEWING PROPOSALS IN ADVANCE OF A MEETING CAN TAKE UPWARDS OF 100 HOURS PREPARATION TIME; THE MEETINGS THEMSELVES TAKE TWO OR THREE DAYS AWAY FROM CAMPUS UP TO THREE TIMES A YEAR; AND THIS VALUABLE VOLUNTARY SERVICE TO THE GOVERNMENT AND THE COMMUNITY IS TOO RARELY RECOGNIZED BY THEIR OWN INSTITUTION.

TO DEAL WITH THIS ISSUE, MR. BLOCH URGED THE PRESIDENTS TO DECIDE WHAT SORT OF HELP THEY CAN PROVIDE SUCH AS REDUCING TEACHING LOADS DURING THE REVIEW PERIOD AND PROVIDING POSITIVE INSTITUTIONAL RECOGNITION INCLUDING EXPLICIT CREDIT TOWARDS PROMOTION. FOR OUR PART, WE ARE PREPARED TO GIVE THEM RELATIVELY LIGHTER REVIEW ASSIGNMENTS OR ACCEPT PARTIAL PARTICIPATION SUCH AS ATTENDING ONLY ONE MEETING A YEAR. ALTHOUGH THESE LETTERS WERE MAILED IN THE LAST THREE WEEKS, WE HAVE ALREADY RECEIVED SEVERAL FAVORABLE REPLIES AND TELEPHONE CALLS.

WITH REGARD TO THE WORKLOAD ON APPLICANTS, NSF HAS TAKEN THE LEAD AMONG FEDERAL AGENCIES FOR MANY YEARS TO STREAMLINE REQUIREMENTS, ELIMINATE PAPERWORK, AND SPEED UP THE DECISIONMAKING PROCESS. IN JANUARY WE INITIATED A MAJOR TRIAL OF A NEW GRANTS MANAGEMENT PROCESS IN FLORIDA; I BELIEVE THE COMMITTEE IS ALREADY FAMILIAR WITH THAT. ALSO, EARLIER THIS YEAR THE DIRECTOR REDUCED THE DEADLINE FOR INTERNAL HANDLING OF PROPOSALS BY PROGRAM OFFICERS FROM NINE MONTHS TO SIX MONTHS. THIS ONE-THIRD REDUCTION IN THE TIME PERMITTED IS A CHALLENGE, CONSIDERING THE AMOUNT OF TIME IT TAKES TO SOLICIT AND RECEIVE REVIEWS, OPERATE PANELS, AND MAKE REASONED AND BALANCED RECOMMENDATIONS.

FINALLY, WE HAVE INITIATED TWO PARTICULAR MEASURES TO ENCOURAGE AND RESPOND TO ESPECIALLY INNOVATIVE, HIGH RISK RESEARCH PROPOSALS. OUR ENGINEERING DIRECTORATE UNDER DR. NAM SUH PROPOSED BOTH OF THESE EFFORTS AND IS NOW CARRYING THEM OUT.

THE FIRST, WHICH BEGAN IN MARCH 1985, IS A SPECIAL CONSIDERATION PROCESS FOR ENGINEERING RESEARCH PROPOSALS THAT CAN BE CHARACTERIZED AS HIGH RISK/HIGH RETURN, TRULY EXCEPTIONAL, OR NOT FITTING WITHIN THE EXISTING PROGRAM STRUCTURE.

THIS PROCESS IS FOR PROPOSALS THAT HAVE BEEN PEER REVIEWED IN THE REGULAR MANNER. BOTH APPLICANTS AND REVIEWERS ARE AWARE OF THE INTERNAL CONSIDERATION PROCESS AND THEY ARE ASKED TO IDENTIFY ELEMENTS OF PROPOSALS THAT FALL WITHIN THOSE CATEGORIES. A PORTION OF THE DIRECTORATE'S BUDGET IS RESERVED FOR THIS PURPOSE.

IN ITS YEAR OF OPERATION, THE DIRECTORATE'S "COMMITEE ON INNOVATIVE AND EXCEPTIONAL RESEARCH" HAS JOINTLY FUNDED 19 PROPOSALS ACROSS ALL AREAS OF ENGINEERING RESEARCH.

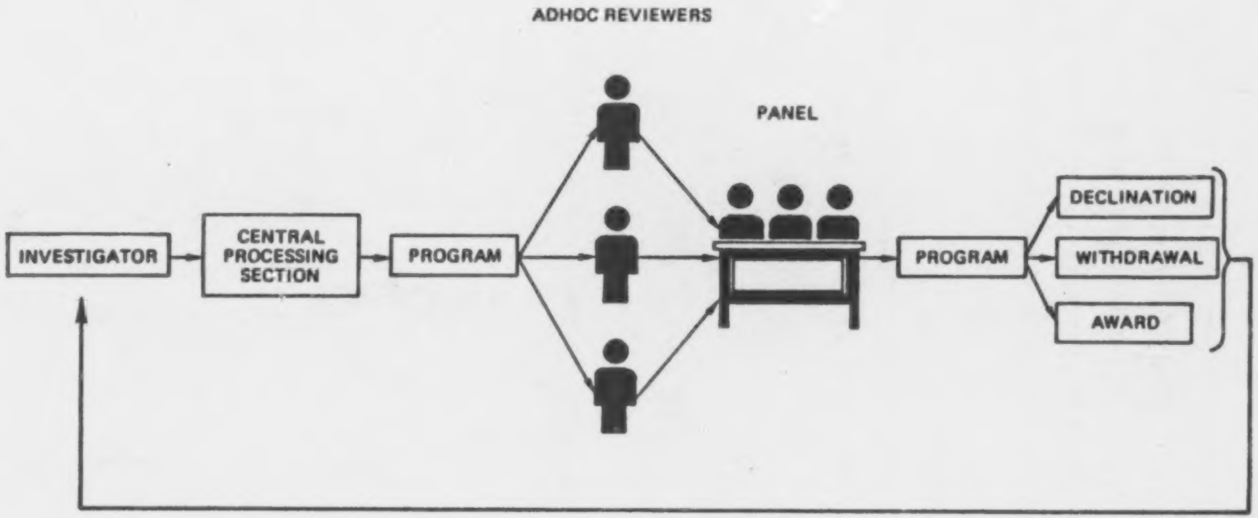
THE MOST RECENT DEVELOPMENT TO INCREASE THE RESPONSIVENESS OF THE SYSTEM TO ESPECIALLY INNOVATIVE IDEAS IS THE ENGINEERING DIRECTORATE'S EXPERIMENT TO PROVIDE SMALL AMOUNTS OF FUNDS FOR NOVEL RESEARCH IDEAS, BASED ON THE PROGRAM OFFICER'S DETERMINATION; THAT IS, WITHOUT EXTERNAL REVIEW. THE DIRECTORATE ANNOUNCED IN DECEMBER THAT IT WILL PROVIDE A LIMITED AMOUNT OF SUPPORT FOR PROPOSALS OF $30,000 OR LESS DESIGNED TO CAPITALIZE ON OPPORTUNITIES FOR EXPLORATORY RESEARCH, PARTICULARLY IN EMERGING AREAS AND FOR MULTIDISCIPLINARY WORK.

THESE PROPOSALS ARE VERY BRIEF AND THE GOAL FOR TURNAROUND TIME IS LESS THAN ONE MONTH. I SHOULD EMPHASIZE THAT AWARDS ARE SMALL

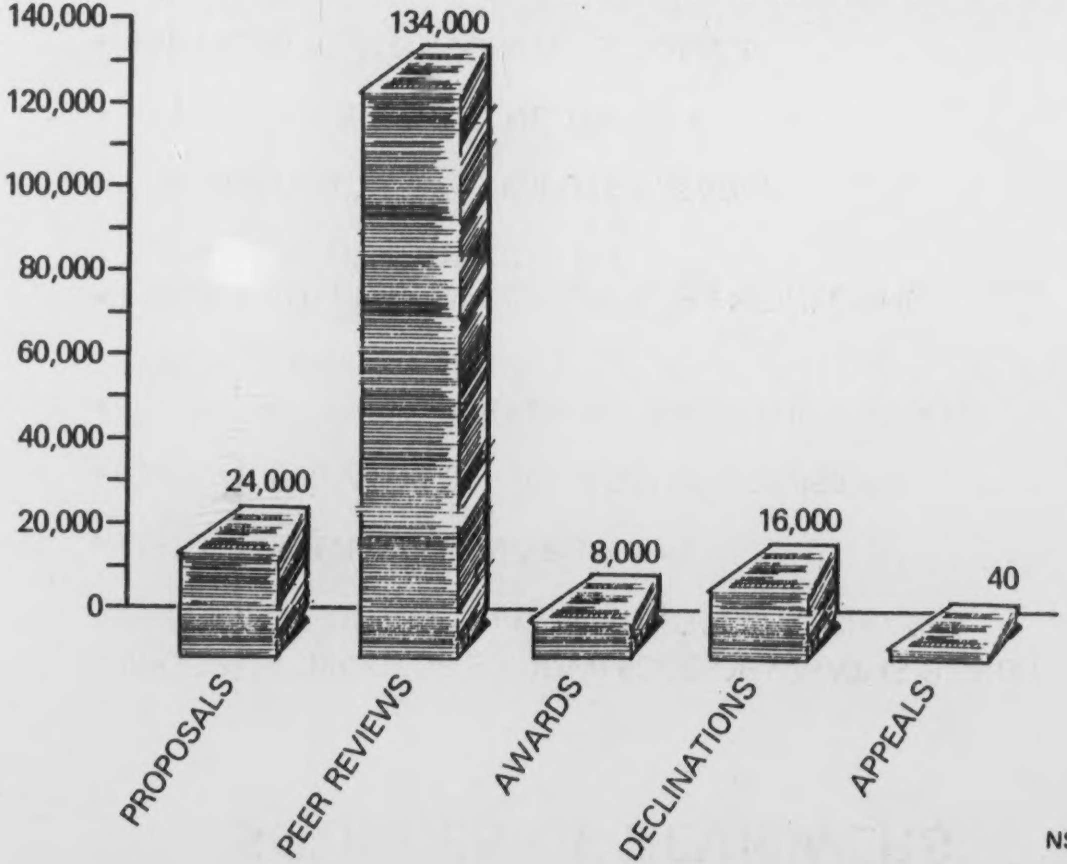
AMOUNTS OF FUNDS FOR NO MORE THAN ONE YEAR OF WORK. CONTINUATION OF ANY OF THE PROJECTS WILL REQUIRE A COMPLETE REGULAR PROPOSAL, TO UNDERGO FULL REVIEW. IN THE FIRST FEW WEEKS OF THE PROGRAM'S EXISTENCE, TEN OF THESE AWARDS HAVE BEEN RECOMMENDED. AS YOU CAN IMAGINE, THE INTEREST IN THE ENGINEERING RESEARCH COMMUNITY IS VERY HIGH AND THE PROGRAM OFFICERS FIND THEMSELVES BUSIER THAN EVER.

THAT COMPLETES MY FORMAL STATEMENT, MR. CHAIRMAN. I SHALL BE GLAD TO ANSWER ANY QUESTIONS YOU OR OTHER MEMBERS OF THE TASK FORCE MAY HAVE.

THE REVIEW PROCESS


ADHOC REVIEWERS
PANEL
INVESTIGATOR
CENTRAL PROCESSING SECTION
PROGRAM
PROGRAM
DECLINATION
WITHDRAWAL
AWARD

FY 1985 COMPLETED PROPOSAL ACTIONS



NSF/86

SOURCES OF REVIEWERS

- PROGRAM DIRECTOR'S KNOWLEDGE OF WHAT IS BEING DONE AND WHO'S DOING WHAT IN THAT RESEARCH AREA
- LIST OF REFERENCES IN PROPOSAL
- FILE OF REVIEWERS IN THE RESEARCH DIVISIONS
- RECENT TECHNICAL PROGRAMS FROM THE VARIOUS PROFESSIONAL SOCIETIES
- RECENT AUTHORS IN SCIENTIFIC & ENGINEERING JOURNALS, QUARTERLIES, ETC.
- S&E ABSTRACTS BY COMPUTER SEARCH
- REVIEWERS' RECOMMENDATIONS
- AMERICAN MEN & WOMEN OF SCIENCE

NSF/86

RESTRICTIONS ON USE OF REVIEWERS

- NO CONFLICT OF INTEREST WITH PI
- NO PENDING PROPOSALS IN SAME AREA WITH NSF
- NO RECENT DECLINATIONS FROM NSF
- DO NOT USE TOO OFTEN

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

nsf

OFFICE OF THE
DIRECTOR

MAR 19 1986

Dr. S. Frederick Starr
President
Oberlin College
Oberlin, Ohio 44074

Dear Dr. Starr:

I am writing to you as President of one of our nation's predominantly undergraduate institutions to enlist your help in increasing participation of four-year colleges on advisory committees and panels of the National Science Foundation.

Over the past weeks I have been reviewing the membership of the Foundation's advisory committees and peer review panels. In general, I have found the composition of the groups to be a fair representation of the science and engineering communities served by NSF. However, I know we would add even greater breadth to our committees if we appointed additional representatives from predominantly undergraduate institutions. I feel sure you will agree with that conclusion. I also know this is in line with the thinking expressed in the "Oberlin 48" report and the testimony of a number of witnesses at the National Science Board hearings on undergraduate education.

The situation is somewhat more complex than the simple addition of more representatives from your institution. For example, despite the honor of being selected, all of your faculty members might not greet peer review panel service with great enthusiasm. From their point of view there are some serious drawbacks. First, reviewing proposals takes upwards of 100 hours preparation time. Secondly, panel meetings take 2-3 days away from campus up to three times per year. Finally, and perhaps most significantly, this activity is rarely recognized by the home institution in a way beneficial to the scientist or engineer. In fact, a scientist with heavy teaching responsibilities might be adversely affected by accepting a committee or panel appointment.

It is at this point that I need your assistance. Institutions must recognize the importance of this service to the science and engineering community, to the institution itself and as a bona fide learning experience for the individual. From our end I propose that we ask panel members to attend only one meeting per year and that we give lighter review assignments to members with heavy teaching commitments, which, by the way includes some members from universities. NSF, of course, will pay for travel, $100 per day reimbursement for expenses during committee meetings and $100 per day honorarium.

You will have to decide what assistance you can provide. I suggest that a reduced teaching obligation during the review period would be very helpful. Further, institutional recognition such as credit towards promotion or other advancement might be considered. I am sure you can think of additional or other means of positive reinforcement.

I am interested in your reaction to my proposal and would welcome your comments and suggestions.

Sincerely,

Erich Bloch
Director

Mr. **Fuqua**. Thank you very much.

Before we begin with Dr. Berliner, we have a vote on the floor and we're going to have to take a short recess while we go over and vote. Then we'll be right back and resume.

[Whereupon, the task force was in short recess.]

Mr. **Fuqua**. If we could take our seats, we will begin or resume the hearing this morning with Dr. Robert Berliner, dean emeritus and professor of physiology and internal medicine at Yale University, and former Deputy Director of Science at NIH. Dr. Berliner, we're very happy to have you.

STATEMENT OF DR. ROBERT W. BERLINER, PROFESSOR EMERITUS OF PHYSIOLOGY AND INTERNAL MEDICINE, YALE UNIVERSITY, NEW HAVEN, CT; AND FORMER DEPUTY DIRECTOR FOR SCIENCE, NATIONAL INSTITUTES OF HEALTH, U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES

Dr. **Berliner**. Thank you. Mr. Chairman, with your indulgence, I will stick pretty close to my written testimony, which is fairly brief.

I believe you heard yesterday from Dr. Ruth Kirschstein of NIGMS, and so the aspects of project selection which correspond more closely to what you've heard this morning would have been covered in that.

In talking about the project selection in the intramural program at NIH, which is what I've been asked to do, it's rather difficult to specify it in those terms because the intramural NIH system in which it works doesn't involve the selection of projects as such. In general, it involves the selection of people who are going to do the research and the general areas they are to explore.

So I will say something about how the work of intramural science is oriented and evaluated and point out some of the differences in the conditions under which they work, between intramural scientists and those, largely in the universities, whose research is supported for the most part by the NIH extramural program, and point out some of the accomplishments and contributions of the intramural program to progress in the biomedical sciences and to the training of those who assume leadership roles in biomedical science at NIH and elsewhere.

I believe that the data provide evidence that although there is, strictly speaking, no project selection—one might even say no projects as such—the system is, on the whole, highly effective in turning out work that is influential in furthering the goals for which NIH was established and in producing leaders for future work in the field.

It might be said that the first step in determining the areas of research to be explored in the NIH intramural program is taken by the Congress in establishing the various categorical Institutes. Each categorical Institute has a serious commitment to establishing research programs in areas that are likely to produce results that will further their categorical goals.

One of the major responsibilities of the directors of intramural research of each of the Institutes is to select areas of research that are relevant to the categorical goals of that Institute and that seem ripe for productive exploration and then to find first-rate scientists whose interests are focused in the selected areas to provide leadership for the work in this field. In turn, the work of the major unit, which at NIH is called the laboratory, may be divided among several sections, each of which is led by an individual of established scientific accomplishment, and finally, within sections, there are likely to be a number of more or less independent scientific investigators under the guidance of the head of the section.

The resources available to support the intramural research in each of the Institutes are apportioned among the laboratories, according to the judgment of the Director of Intramural Research and within each laboratory in accord with the judgment of its chief. It is largely through the allocation of resources—that is, space, positions, and funds for supplies and equipment—that control is exerted, that good work and productivity are rewarded and unfruitful efforts discouraged.

However, unlike the usual procedures of project selection in the NIH extramural program, with formal project proposals and review, investigators in the intramural program are not generally required to set forth detailed plans for what they proposed to undertake. Plans are discussed on a much more informal basis with their supervisors and investigators are encouraged to pursue their research and to take advantage of new investigative opportunities as they arise. To a certain extent, intramural scientists may be more venturesome in selecting their research pursuits than might be the case if they must formally justify their plans and were they to feel totally dependent upon early positive results to justify renewed support for their efforts. However, the prevalence of major risk taking, especially by more junior members of the staff, is limited, since they, too, must show evidence of productivity if they are to gain permanent status within the organization, or to qualify for good positions elsewhere, or if they are to gain promotion within their Institutes and to achieve increases in the resources at their disposal. I think in the intramural program, as everywhere else, it's largely the senior, well-established individuals who can really afford to take chances.

Given the relative freedom with which intramural scientists may pursue their research, it is obviously critical that those in positions of scientific leadership be highly qualified for their roles. In the earlier days of the growth of NIH, scientists already demonstrated to be of the highest investigative competence could be relatively easily recruited to the NIH intramural program which offered opportunities for productive research that were very scarce elsewhere. As the availability of NIH support extramurally grew, however, the latter advantage largely disappeared and this, combined with the generally unfavorable salary differentials, and the greater restrictions that go with Government employment, has made it very difficult to recruit scientific leaders from outside. Quite to the contrary, the growth of the intramural NIH as an unparalleled site for the advanced training of research scientists has made it possible not only for NIH to develop its own intramural leadership talent, but also to serve as a source of recruits for leadership roles in other institutions.

Of course, high standards must be applied not only to the selection of scientific leaders, or those in intermediate leadership positions, but also to the admission of new investigators to the permanent staff at more junior levels. For this purpose, the usual 1-year probation period that is standard for the civil service is obviously quite unsuitable and means have been found, through the staff fellowship program at NIH, to extend the period before a decision on offering permanent status up to 7 years, a time comparable to the time to tenure decisions in most universities.

The decision to offer permanent status is based on a review of the individual's productivity and contributions to his or her field, and that includes the solicitation of outside opinions and is based on recommendations of the scientific directors of all the Institutes acting as a promotions committee. Not only is this award of tenure, we might call, based on the recommendation of this group, but promotion of all senior research personnel is subject to its review and approval.

In addition to continuing internal review of the work of individual investigators and research groups in a somewhat more hierarchical system than generally exists in the university setting, there is a system of periodic review by groups of outside experts who constitute the boards of scientific counselors. These groups, supplemented, if necessary, with ad hoc members with special expertise, review in turn the work of each of the activities in the intramural programs of each of the Institutes and make comments and recommendations concerning the quality and direction of the research and any needed changes.

I would be somewhat ingenuous if I did not recognize that there is, in some quarters, considerable envious resentment of the freedom of NIH intramural scientists from the requirement repeatedly to apply formally for support of their research, as is the case in most of the outside world. On the other hand, few of those who are most vociferous in their criticism would be willing to change places with the intramural scientists; the traffic is almost entirely in the other direction. This is because the freedom from applying for research support, along with the ability to pursue their research almost without distraction by other responsibilities, is about the only advantage accruing to intramural NIH.

In general, those in universities are higher paid and, in particular, are likely to receive educational benefits for their children—several NIH scientists have found it in their interest to leave at the time their children get to about college age because they can get that kind of support outside and not at NIH—and they also have greater freedom to engage in outside activities, some of which may be highly remunerative, and have open to them multiple sources of funding.

In addition, the NIH laboratories are by far the most crowed that I think I have ever seen. Most intramural scientists envy the advantages of those on the outside. They, too, would like to have the best of both worlds. But those that do stay at NIH do so because it is a wonderful place to work and to accomplish science.

The important question to be answered is not whether the NIH intramural system is unfair to those on the outside, but whether it is effective and efficient in advancing the goals for which the institutes were established. Does the productivity of the intramural research program and the contribution of intramural NIH to the training of the Nation's biomedical research manpower indicate that the system under which it operates is effective?

All the objective evidence would suggest that the quality of the work at NIH rates very high in the world of biomedical research. The source of much of this evidence is the *Citation Index* which keeps track of the number of times a given paper is referred to by authors of subsequent papers. In general, papers are cited because they have contributed significant information to their fields and have thus contributed to further advances. The more often a paper is cited, the more likely that it has represented a significant advance and the more important the work it described was to its field.

A study in 1975 of the total annual production of papers in biomedical research journals found that intramural NIH was second nationally, in the total number of papers published—first place was occupied by the University of California, which included all of its campuses. When the importance of papers was estimated by weighting for the influence of the journal in which they were published, the importance of the average publication from NIH ranked second nationally behind only those from Harvard. A little over 3 percent of American biomedical scientists are estimated to be in the NIH intramural program, but they provided a little over 5 percent of the publications in the most influential journals.

A review of the most frequently cited authors during the period 1961 to 1976 revealed that 31 of the 300 most cited scientists were working in the NIH intramural program when the list was published in 1978. This was expanded in 1981 to the 1,000 most often cited, among whom there were 86 at NIH. It is important to note that the literature covered by this list includes all of the so-called hard sciences, among which are a number such as physics, geology, astronomy, geophysics, and so on, not represented at NIH at all. If there are, as has been estimated, about a million working scientists worldwide, about 0.15 percent of them work at NIH but contribute 8½ percent of the most quoted papers. Or, looked at in another way, about 4 percent of U.S. expenditures for biomedical research goes to intramural NIH, and probably about 2 percent of the worldwide expenditures, and this yields about 8 percent of the most influential work.

There are other indications of the high quality of the NIH intramural scientists and their work and the high regard in which they are held by fellow scientists, such as the receipt of Nobel Prizes and other awards, and election to the National Academy of Sciences—and I won't go into detail here. But there can be no doubt that the quality of work produced warrants the freedom of research directions that lies behind it.

In addition to its role as a contributor to biomedical science, NIH has been an important site for the training of research manpower. A list of the alumni of NIH intramural programs who hold important positions in academic medicine, particularly in the more research-oriented schools, and in other research institutions, would be a long one. At the more general level, a recent study of the role of intramural NIH in the orientation of M.D.'s to research careers suggests that the experience at NIH has a powerful influence.

You are probably aware of the fact that there has been considerable concern expressed about the decreasing proportion of M.D.'s among recipients of NIH research grants. The survey compared M.D.'s with other equivalent—who had otherwise equivalent training, who had had or had not spent time in intramural NIH. Among those who had had fellowships through the NIH extramural programs, 59 percent had subsequently been principal investigators on research grants if they had been in the intramural program. If they had had fellowships but had not been at NIH, only 26 percent ended up as grant recipients. Among those who had not received fellowships, 25 percent of NIH alumni subsequently received research grants, compared with a little less than 2 percent of those who did not have this experience. I think it is apparent NIH must be doing something right.

I'm glad to have this opportunity to talk about the NIH intramural program, which I have been disassociated from for the last 12 years, because I still have the highest regard for it, and I hope I will be able to answer any questions you may have.

[The prepared statement of Dr. Berliner follows:]

Statement of Robert W. Berliner, M.D.

before

Science Policy Taskforce

Committee on Science and Technology

U.S. House of Representatives

I am Dr. Robert Berliner, currently Professor of Physiology and Internal Medicine (Emeritus) and formerly Dean of the School of Medicine of Yale University. I was involved with the Intramural Program of National Institutes of Health for some 23 years. I joined what was then the National Heart Institute in 1950 as Chief of the Laboratory of Kidney and Electrolyte Metabolism, a role I maintained until 1962. In 1954 I added to that responsibility the Directorship of Intramural Research of the Heart Institute. I left the Heart Institute in 1968 to become Director of Laboratories and Clinics in the office of the NIH Director, with responsibility for coordinating the intramural research activities of all of the Institutes. A year later to those were added additional responsibilities in the position of Deputy Director for Science a role that I held until 1973 when I left NIH to become Dean at Yale. I hope you will forgive this recital which repeats material already available to you but I believe it is helpful in establishing my intimate familiarity with the intramural activity at all of its levels. Although I have not been formally associated with NIH for something more than 12 years, I have maintained contact with many of my former colleagues there. Thus, I have been away long enough to be able to look at NIH from a different viewpoint and possibly somewhat more objectively. At the same time I have kept sufficiently well-informed to know that things are done in much the same way in the Intramural Program now as they were when I was a part of it.

I was asked to speak about "Project Selection" in the intramural program, the subject being symmetrical with that covered by other witnesses. It is impossible, however, to specify the process of project selection in intramural NIH because the system by which the work at NIH is directed does not involve the selection of projects as such, but rather the selection of people and the general areas that they are to explore. I can and will,therefore, say something about how the work of intramural scientists is oriented and evaluated, point out some of the differences, in the conditions under which they work, between intramural scientists and those, largely in universities, whose research is supported for the most part by the NIH extramural program, and point out some of the accomplishments and contributions of the intramural program to progress in the biomedical sciences and to the training of those who assume leadership roles in biomedical science at NIH and elsewhere. I believe that the data provide evidence that although there is strictly speaking no project selection - one might even say no projects as such - the system is, on the whole, highly effective in turning out work that is influential in furthering the goals for which NIH was established and in producing leaders for future work in the field.

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One of the major responsibilities of the directors of intramural research of each of the institutes is to select areas of research that are relevant to the categorical goals of that institute and that seem ripe for productive exploration and then to find first rate scientists whose interests are focused in the selected areas to provide leadership for the work in this field. In turn the work of the major unit, the laboratory, may be divided among several Sections, each led by an individual of established scientific accomplishment, and finally, within Sections, there are likely to be a number of more or less independent scientific investigators under the guidance of the head of the Section. The resources available to support the intramural research in each of the Institutes is apportioned among the Laboratories, according to the judgment of the Director of Intramural Research and within each laboratory in accord with the judgment of its Chief. It is largely through the allocation of resources, that is space, positions, and funds for supplies and equipment, that control is exerted, that good work and productivity are rewarded, and unfruitful efforts discouraged.

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Given the relative freedom with which intramural scientists may pursue their research it is obviously critical that those in positions of scientific leadership be highly qualified for their roles. In the earlier days of the growth of NIH, scientists already demonstrated to be of the highest investigative competence could be relatively easily recruited to the NIH intramural program which offered opportunities for productive research that were very scarce elsewhere. As the availability of NIH support extramurally grew, however, the latter advantage disappeared and this, combined with generally unfavorable salary differentials and the greater restrictions that go with government employment, has made very difficult the recruitment of scientific leaders from outside. Quite to the contrary, the growth of intramural NIH as an unparalleled site for the advanced training of research scientists has made it possible not only for NIH to develop its own intramural leadership talent but also to serve as a source of recruits for leadership roles in other institutions.

Of course high standards must be applied not only to the selection of scientific leaders or those in intermediate leadership positions, but also to the admission of new investigators to the permanent staff at more junior levels. For this purpose the usual one year probation period that is standard for the civil service is obviously quite unsuitable and means have been found, through the Staff Fellowship program, to extend the period before a decision on offering permanent status up to seven years, to a time comparable to the time to tenure decisions in most universities. The decision to offer permanent status is based on a review of the individual's productivity and contributions to his or her field that includes the solicitation of outside opinions and is based on recommendations of the scientific directors of all the institutes acting as a promotions committee. Not only is this award of "tenure" based on the recommendation of this group but the promotion of all senior research personnel is subject to its review and approval.

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The important question to be answered is not whether the NIH intramural system is unfair to those on the outside but whether it is effective and efficient in advancing the goals for which the institutes were established. Does the productivity of the intramural research program and the contribution of intramural NIH to the training of the nation's biomedical research manpower indicate that the system under which it operates is effective?

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Thank you for this opportunity to speak about the NIH intramural program, about which, it is apparent, I have the highest regard. I will try to answer any questions you may have.

PANEL DISCUSSION

Mr. **Fuqua**. Thank you very much, Dr. Berliner.

Having been at NIH and now on the other side, or having been dean of a medical school, where you, I am sure, applied for grants at NIH, do you see the method as effective as when you were administrating it, or as when you have been receiving the grants? I'm not speaking of you personally but the department that you headed.

Dr. **Berliner**. Well, of course, I didn't personally apply for any grants. A dean doesn't get a chance to do much research.

Mr. **Fuqua**. I realize that. But did your associates?

Dr. **Berliner**. Well, I think the system is a good one. I think the system, the intramural program, is not one that can easily be extended to other institutions. As a matter of fact, I believe that most of the investigators around the country would prefer to do negotiating for their money for their research with NIH than they would with their own administration. I think they would feel that the more impersonal aspects that go with the review through the extramural program probably suits them better than if there were a single pot of money given to, say, Yale University for the dean to dispense at his judgment.

Mr. **Fuqua**. One of the claims that has been made of the present peer review system, by the way it's currently designed, is, that it is not suitable for the selection of interdisciplinary projects.

Would any of you care to comment on that?

Dr. **Clutter**. I could comment on that.

Yes, there are some problems with selecting some interdisciplinary or multidisciplinary projects, but in my testimony I alluded to the way we're handling it in the Foundation. And that way is to have program officers, across programs and across disciplines, coreview a proposal. Sometimes the proposals themselves are constructed in a way that makes them very difficult to review by single bodies.

We have just started a program last year, an interdisciplinary program—in fact, we've got a number of them—but one across the life sciences and chemistry that's called the chemistry of life processes. The way we select those proposals is to do the standard kind of mail review, panel review, but then we call in a special interdisciplinary panel to review all of the proposals and to help us make selections. That seems to be working effectively.

Mr. **Fuqua**. Who makes the ultimate decision?

Dr. **Clutter**. Who makes the ultimate decision? The group of program officers involved across the disciplines. And, of course, we have the Engineering Research Centers, which are multidisciplinary, and that involves a very extensive review process which includes site visits with multidisciplinary teams of scientists and engineers reviewing the projects. So that I think that, while it's difficult, it's not impossible.

Mr. **Fuqua**. Dr. Moss, would you or Dr. Kane care to comment on that? You're probably more involved in multidisciplinary programs than maybe specifically NSF would be.

Dr. **Moss**. Mr. Chairman, I agree with the comments that Dr. Clutter has forwarded here. We are involved a lot, and we do much the same as I hear described at NSF. We will have projects, for example, human-machine interface where we have computer science even related to artificial intelligence, but really the psychology and training at the interface there. We bring a team together from the life sciences, from the computer sciences, or information sciences, and they work together. We establish the thrust and then they solicit proposals in the area and they are decided usually as a team. I think that's what I hear at NSF.

Mr. **Fuqua**. Dr. Kane?

Dr. **Kane**. I can't give any magic recipe. I do believe it's a problem, and I'll give an example from my memory which I think illustrates the problem; that is, accomplishment in science, I think, has had as much effect on the way we think in the last 10 years as almost anything that's been done, and that is the catastrophic theory of what caused the end of the dinosaurs.

Now, that project was submitted to every agency in town and was turned down, including my own agency, so I can't say I'm any better than anybody else in this case. It was sent to the paleontologists who said, "That's crazy, you know, it isn't paleontology." It was sent to the physicists, who said, "That's lousy physics." And there it is, something that, in retrospect, has been one of the most exciting things that's happened in science and it got turned down.

Now, how we can keep this from happening again is your real question. I don't know the answer. There is certainly no total recipe. But I think one thing is to get a stable of reviewers. I don't think you can do without peer review because you have to have the judgment of smart people helping you on all this project selection. And so you have to comprise a very carefully selected set of peer reviewers.

Another thing that I put in my written testimony is that you have to be very careful for proposals getting what I call mixed marks, where one person says "This is the most sensational proposal I ever heard," and two other people say it gets a D minus, you know.

Mr. **Fuqua**. I'm not proposing that we eliminate peer review.

Dr. **Kane**. No, I understand.

Mr. **Fuqua**. I'm saying that the peer review system is structured so that people maybe are afraid that they'll get turned down, like the case you cited, if they come in with multidisciplinary proposals, where you have a lot of disciplines involved in something. Are we putting a damper on those type of proposals?

Dr. **Kane**. That particular case sure got a lot of attention. It made us realize the system had flaws in it. Again, my starting remark was I have no magic recipe. But certainly the choice of really wise peer reviewers—and I think the word gets around; I think all my colleagues at the table will admit that there are peer reviewers to whom I would send that kind of proposal. Every official has a little list of these kind of people that are known to be free thinking and very creative. The other thing, I think it mostly boils down to is don't be too sure the system works perfectly and keep a good eye on it.

Mr. **Fuqua**. Of course, peer review is a highly competitive system. You look at the number of proposals that have been made, as was indicated in this chart was one, and awards and the declinations, withdrawals, and so forth. Some have said that this system stifles innovation because some of the scientists are unwilling to share their kind of daring ideas with their competitors.

Is that a valid criticism?

Dr. **Clutter**. I don't know whether you could hear Dr. Berliner's remark, but he said there were apparently people everywhere.

I don't think it's a particular problem, but, of course, it exists. There are people who don't want to share their ideas and so they don't put them into their proposals. But I think that we're inviting people to submit high-risk, innovative proposals. We'll just have to wait and see what happens. We just started this within the last couple of months. I understand that they've already funded 10 of them. It's only been available for about 1 or 2 months.

It could be a problem, but I don't think it's a significant one.

Mr. **Fuqua**. Well, when a reviewer is asked to read a proposal, are they told the institution and investigator?

Dr. **Clutter**. Yes. They are told.

Mr. **Fuqua**. Could that interject bias into their decision?

Dr. **Clutter**. Well, as I said, we have very clearly defined conflict of interest rules.

Mr. **Fuqua**. I'm not talking about a conflict of interest.

Dr. **Clutter**. I know. If a reviewer has a bias, they're supposed to state that they have a bias. Sometimes they might not know that they had a bias. But when you solicit about 6 to 10 reviews, it's pretty easy to spot the one where somebody is biased, and so we simply don't use that review in making our decisions.

Mr. **Fuqua**. Suppose someone is very controversial in their field, have been very forward in some of their thinking, and it's created some controversy in their field. Would someone be prejudiced against them?

Dr. **Clutter**. Yes; that can happen. That absolutely can happen. I guess it happens in every walk of life. If you don't have any friends at all, it's really hard to get along in the world. But it occasionally happens—1 out of 24,000 cases.

Mr. **Fuqua**. At NSF, is the program officer obliged to take the findings of the peer review group, or can he or she make a decision on their own and say, "Well, really, in this case, it was very close; I think this project should get the award."

Dr. **Clutter**. Well, we say that the reviews that we solicit provide advice for the program officer who makes the ultimate recommendation to the Foundation for support of the proposal or declination of the proposal. So that if it appears that all of the reviewers were biased, that program officer does not have to use those reviews. But what would ordinarily happen is that that program officer would send out for more reviews to make sure they had a fair representation of the views of the community.

In that innovative proposal experience that I talked about, we're giving that program officer and the committee the opportunity to make some decisions without external peer review. So we are trying a whole lot of experiments. But you're right, the program officer does not have to use the opinions of the external reviewers.

However, I'll just say parenthetically that that program officer had better make a pretty air-tight case for supporting that particular project before somebody like me is going to say OK.

Mr. **Fuqua**. Yes; Dr. Kane.

Dr. **Kane**. I'd like to point out a situation where this kind of conflict does arise, and I don't believe it's due to any criminal activity or anything like that at all.

It's happened a number of times between university proposals and work at the national laboratories, which operates essentially the same way as Dr. Berliner described in the intramural program. A proposal will be read by a scientist at the laboratory and in it there will be an idea that solves some of the problems that he or she is working on. Now, it isn't plagiarism but it would be very hard for an individual who reads something in a proposal that he or she can immediately apply to their own problems not to use it. The labs have these large resources of turning to a problem immediately—they don't have to write proposals in general to do it. This has happened a number of times, where a very good idea would be floated by in a proposal and an individual at the lab would then use some aspect of that idea to do his or her work. It really gets people upset. I don't know the cure for it. If you were working in a laboratory and you were suddenly apprised of an idea that would help you solve your problem, it would be kind of hard to ignore it. And yet, I've seen this happen. I think it is a very small problem in the context of the whole business, but it would be wrong to say it didn't happen.

Mr. **Fuqua**. On that point, Dr. Kane, there has been some feeling in the university community that when a university applies to a national lab for a grant or for an unsolicited proposal, and the people, the reviewers, are people who may feel like maybe some of the money is coming out of their pocket—I mean, their programs—that they would rather do it, say, at the lab, that there's an inherent conflict of interest or not an arm's length evaluation going on about the project. That could be true probably at the Naval Reseach Lab or maybe in DOE by the way the two agencies are operated.

Could you comment on that?

Dr. **Kane**. Yes; I will. There are parts of the Department of Energy that do use the national laboratories as almost a part of their own organization to help adjudge and grant research. Energy research, which is 90 percent of the basic research within the Department of Energy, does not use that procedure for precisely the reasons that you mentioned. In other words, a university grant in high energy physics does not go through Fermilab or Stanford Linear Accelerator Lab. A university grant in materials science does not go through Argonne or Brookhaven or Los Alamos. It's for precisely the reasons you mentioned this is done.

There is a small amount of work done where the labs actually contract with the universities, but that's usually something where it's mutually agreed it'll help solve a problem, so the lab is actively soliciting help in this case. But I think it's pretty safe to say that for over 90 percent of the Department of Energy's work, we do not use the national labs as the judges, for just the reasons you've gone into. Whether there would be a real conflict or just the perception doesn't make any difference. It would be bad.

Mr. **Fuqua**. Dr. Moss.

Dr. Moss. It is somewhat similar in ONR and in the Navy research laboratories. However, our laboratories' funding are decided at the beginning of the fiscal year—actually, before the beginning of the fiscal year—and the funding that goes to the laboratory funding and the funding that goes to our contract research program which funds the universities in our program are fixed. We feel free, if we think it will help, to go to a laboratory scientist in a given area to ask what his opinion is. But that's always just advice. Even our peer review, as I said, it's just advice.

I am not aware of where there's ever really been a conflict there, but the budget is sort of decided ahead of time so there's really nothing that's really up for grabs.

Mr. FUQUA. Dr. Moss, you commented in your statement about ONR supporting external research through contracts rather than grants. How do you define the difference between a contract as you see them for research versus a grant, say, maybe that NSF or NIH might make?

Dr. Moss. Mr. Chairman, that's a good question. I can't give you a definitive answer because I have that under study actually right now. We do contracts. We use with the universities what we call a short contract form. And it's not really all that different from grants. But we do require—I guess sort of implicit in the terminology with the contract—a report. That report usually takes the form of the papers that were published during the existence of the contract.

Our contract people have been very innovative with the way they treat the university contracts, but the history of ONR has always been that we have predominantly required contracts. I have that under study right now, for the reason that, as you know, with all the competition and contracting and this sort of thing, the procurement advance lead time is increasing, and it's getting longer, and we're looking to streamline and try to get these things out as quickly as we can. So we're studying the grants versus contract right now.

Some grants can be more laborious than our short form contract, but we're looking into it and I really can't give you the definitive answer. We do grants, also. We have a young investigator program that's actually patterned after that at the National Science Foundation. We award grants there. Our university instrumentation program, which DOD puts in $30 million a year just for instrumentation, they are grants. We do a lot of grants. Our fellowships are grants. But by and large, our unsolicited proposals are contracts.

Mr. PACKARD [presiding]. Thank you.

I have the rare privilege of being on the shuttle this morning. I'm shuttling back and forth from one hearing to another. I apologize.

Dr. Clutter, in your chart it graphically points up a question that came up in our hearing yesterday on this subject, and that is that there's a significant number of applications being submitted constantly to a variety of agencies. Here your chart shows, at least in your agency, 24,000 applications with over—well, it's a 2-to-1 success ratio, or 2-to-1 failure ratio, actually.

Dr. CLUTTER. Yes.

Mr. PACKARD. Twice as many failures or declinations as there are awards made.

Is this a waste of scientific minds and talent in preparing all of these applications that are not successful, or that ultimately do not receive funding?

Dr. CLUTTER. Well, I guess I have to say no. It's not a waste. But I think you were probably out of the room when I talked about something that I really don't have in my written statement, and that is about the hidden benefits of this whole peer review system. One of the benefits, one of the major benefits is educational, because whether or not a person receives Federal support, that person receives a critique of their research program—in our case, it would be half a dozen ad hoc reviews from anywhere in the world, from experts, plus the possibility of a summary of a panel that's been convened to deliberate all of the research proposals and a summary of their remarks which helps that principal investigator construct a better research program. And that, in turn, will enable them to submit possibly a better proposal the next time around.

I don't have good numbers available, but I suspect that people who were declined in one particular year probably have a good chance of getting support the next year.

Mr. PACKARD. In preparing applications, to your knowledge, does it occupy a good deal of the time of our experienced scientists, or are the applications often prepared by the emerging young scientists or a panel or a group of people that are not distracted away from some rather significant research activities?

Dr. CLUTTER. I think it is true that people do spend a great deal of time preparing proposals, and one way we've tried to reduce that burden is by limiting the number of pages in a full-blown proposal. But, in addition, we do have two other methods by which an established investigator can get support without preparing a lengthy proposal.

Mr. PACKARD. In those rejects—those that are not funded—do you have any tracking mechanism or data that would indicate whether they are able to find funding from other sources?

Dr. CLUTTER. No, we don't have a really good tracking mechanism for that. However, some program officers do keep informal records on that because occasionally, or very often, people who withdraw their proposals have withdrawn them because they've received funding from another agency, which could be Federal or non-Federal. I would guess that probably about 90 percent of the withdrawals are withdrawn because they've received other support. So we do have a kind of informal tracking system.

Mr. PACKARD. So your general observation would be that of these 16,000 out of 24,000 that are declined, it doesn't mean that they will all be laid on the shelf.

Dr. CLUTTER. Not all of them. That's right.

Mr. PACKARD. OK.

In your review, the program officers or your peer reviewers, do they ever plug the cost of a project into the total review process? Is that a factor, particularly during constrained budget years now? Is there any portion of the formula that does address the economics of projects?

Dr. **Clutter**. Yes. In fact, we ask the reviewers to make comments on the cost of the research project, if they want to. And very often they will. But usually they comment on the cost if it seems excessive, or they may comment on the cost if they think it isn't enough to do the project. Interestingly, that occurs sometimes, too.

Mr. **Packard**. Do you think it would be an appropriate area to actually put some kind of criteria, financial criteria, into the peer review process and into the formula that might consider having research dollars go further, or do you feel that would not demean, but certainly would put a refocusing in the scientific research area that might be detrimental to the research innovation concepts?

Dr. **Clutter**. Well, I think there's an appropriate place for that, and I don't think it's in the peer review itself. In peer review we're really asking for a technical review of the project, and we invite comments if people think the budget doesn't look appropriate. But I really prefer to have the scientific comments. Then our program officers and the staff in the Foundation evaluate the cost of the research, and they're pretty good at that.

Mr. **Packard**. Dr. Kane.

Dr. **Kane**. Could I make a comment on that?

Mr. **Packard**. Oh, please.

Dr. **Kane**. I have feelings about that cost, and they're very strong and very negative. I think it's perfectly appropriate to ask about costs—you know, the person asks for equipment they really don't need, or too much foreign travel or something like that, that's fine. But I'm against too much of an evaluation of costs because it gives the implication we're buying piece work, whose product is known. I just really think that's the wrong approach to science.

If we get scientists bidding against each other for how much they produce per dollar, it seems to me it would have a terribly negative damper on true creativity.

My idea of a good proposal is one that you don't know what the fellow is going to get until he's finished doing it, not one that he promises ahead of time not only will he deliver but he or she will deliver at a price. I just think that's pushing the system too far.

Mr. **Packard**. Do you feel the same on all aspects of science, pure and basic research versus project research?

Dr. **Kane**. No, I can see projects where an estimate of costs would be very realistic to do. In the Department of Energy we do a lot of what I call "bread and butter" science. For instance, measuring cross sections, which are necessary for nuclear reactors might be a good example of that. That's one that's really amenable. It's not creative science; it's necessary, but not terribly creative. I could see where a good cost estimate on that is a very proper thing.

Mr. **Packard**. Dr. Kane, let me pursue another point with you.

In your testimony you argued in favor of lengthening the duration of the awards as a method of reducing the burdens, the administrative burdens. I think that's very understandable.

Why should we not fund researchers on the basis of their track record rather than fund individual projects if, in fact, we're buying brains? In other words, as I think your statement said not a specific piece of preconceived research. Why should we not fund the researchers instead of the project?

Dr. **Kane**. I think that's an excellent idea. I'm reluctant to sit here and try to devise a system, but it seems to me like longer terms and then renewal terms, not too often, and based on progress rather than prospective proposals, would be an excellent idea.

I think the scientific community has to watch out or it's going to get itself into a very strange situation, where it says, "I'm better at judging the promise of research than I am judging the accomplishment of research." I guess I have a hard time thinking that I'm better personally at judging the promise of something than I am of what's been accomplished. So it seems to me like what you suggest is very reasonable, that the term should be long and renewal should be based on accomplishment rather than proposal.

Maybe my colleagues won't agree with me.

Mr. **Packard**. Any other comments on that?

The peer review system is, admittedly, very highly competitive. That's I considered one of its positive features, that competitiveness in the process is healthy. Some have charged that this very competition stifles innovation of our scientists, and that one of the reasons it does stifle innovation is that they may be reluctant to reveal some of their novel and innovative ideas in an application, in a competitive application, and therefore they would not choose to even apply.

Do you have a comment on that, any of the panel?

Dr. **Kane**. We covered that perhaps when you were out.

Mr. **Packard**. Oh. I apologize.

Dr. **Kane**. We discussed that subject. We had no solution, but I think we all agreed that it wasn't as big a problem as it's perceived.

Mr. **Packard**. Well, if it's been covered, I won't ask for further comment on it.

Dr. Moss, you noted that the ONR's scientific officers have the option to call on academic peer reviewers if they conclude that it would be helpful in making a decision.

In actual practice, how does that occur, or how often does it occur, and if it does, does it occur more often in some fields than it does others?

Dr. **Moss**. Congressman Packard, yes, it does occur more often in some fields than others. The way that occurs is that if the scientific officer feels that he needs external peer review, he has full authority to go get that review. It's advisory to him when he gets it, and he uses it in making his decision as to whether he funds the unsolicited proposal.

Formally, peer review is used at ONR on 5 to 10 percent of our unsolicited proposals. We, however, Congressman, we have three things to consider when we fund a proposal, whereas at, for example, NSF, that's not the case. First, we have the scientific excellence of the proposed research. Then we have to consider the relevance of that research to our mission, and we also have to consider whether that research is something that's going to be transitioned from our basic research to exploratory development—to higher categories in our funding. So we have really sort of a three-dimensional system in a way whereas I think NSF, in their broad funding of science, have a one-dimensional system. I'm not trying to make it sound simplistic compared to ours, but we have other considerations.

University scientists cannot really judge whether it's relevant to the Navy, whether it has a potential for being transitioned from 6.1 to 6.2 to 6.3. And that's probably the reason we have such a low percentage of formal peer review. We have a high percentage of informal discussion, of course.

Mr. PACKARD. You described this system that's used by ONR to evaluate your research options including panels of experts. To what extent do you follow the advice of your expert panels? Are they often rejected and do you seek for advice and counsel from both scientists as well as nonscientists when you go outside?

Dr. MOSS. We take the advice of the panel of experts very seriously and at times we will change a priority order, but not very often. It can be done. First of all, it's advisory to us. But, for example, in our program for fiscal year 1988, which we're considering right now, we have 30-some thrust areas. We'll probably end up having enough funds to fund maybe 18 or 20 of those with our turnover in our program, and the priority rating that we get from those panels is very seldom ever, ever changed. It comes to me, and it comes to a corporate board that we have, who has the final decision, and some things are changed there from time to time. But I would venture to say it's much less than 10 percent—maybe 5 percent of the time.

Your other question was—I'm sorry, sir. I think I answered your first one.

Mr. PACKARD. Simply do you go outside very often for expert advice, and if so, do you use your nonscientists as well as your scientists?

Dr. MOSS. When we go outside for advice, if we do peer review, it's usually on the science merit or the quality of the science, as NSF would do. We do use nonscientists, but that's on the Navy relevance. In fact, there it is usually related to naval officers that are very familiar with the problems and the mission and we will go to them at times. But that's really the only place that we use any nonscientists.

Mr. PACKARD. Thank you.

Dr. Clutter, almost all of NSF's grants go to scientists in universities. In fact, I understand that you have specific rules that preclude or at least limit grants to industry, scientists in industry, unless there's some unusual or overriding circumstances.

How does this square with the stated objective of having an open competition and of seeking the highest quality of research proposals? Certainly they're not all concentrated in the universities.

Dr. CLUTTER. Well, the rules you're talking about are old rules in the Foundation and those have been changed. So that scientists from industry can submit proposals and, in fact, we do have the Small Business Innovative Research Program, and in that program all of the people who submit proposals are in small business, small industry.

But scientists from larger corporations do not submit proposals, not because they could not, but because their corporations don't want them to submit proposals to the Federal Government for proprietary reasons. So I don't think we have a closed door.

Mr. **Packard**. Where the proprietary issue becomes a factor in whether they do or do not submit a proposal, is there any cooperative mechanism whereby information is shared?

Dr. **Clutter**. Yes. There's a good example of that that I can give you, in our Engineering Research Centers Program. Because we encourage scientists from industry to collaborate with university scientists, the universities submit the proposals for the whole center program and they invite industry scientists to come in and work with them in collaboration, so the industry scientist doesn't submit a proposal, per se, but they take an active part in the research program. That occurs in several other of the programs in the Foundation as well.

Mr. **Packard**. Do you feel that there is an inherent bias, however, in the granting of applications to universities in preference to industry, or do you feel that scientists from industry would have a fairly equal playing field, level playing field?

Dr. **Clutter**. Well, I have to be honest. I have never ever seen a proposal from a scientist in industry except for the small business program.

Mr. **Packard**. I don't have any further questions. I think that will conclude, without any other members here. I sincerely appreciate each of your testimonies and your answers to the questions and your appearance here at the committee.

If there is no further comments from any member of the panelists, we will adjourn this hearing. Thank you very much.

[Whereupon, at 11:12 a.m., the task force was adjourned to reconvening at 9 a.m. on Thursday, April 10, 1986.]

RESEARCH PROJECT SELECTION

THURSDAY, APRIL 10, 1986

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to call, at 9:05 a.m., in room 2325, Rayburn House Office Building, Hon. Ron Packard (acting chairman of the task force) presiding.

Mr. Packard. Ladies and gentlemen, let's call the hearing to order this morning. We welcome you here.

Procedurally, I would like to ask unanimous consent—and that's rather easy inasmuch as I'm the only one here—to allow for these proceedings to be recorded by photography or videotape. We have photographers here that wish to be able to participate in the hearing.

Ladies and gentlemen, today is the final day of our series of hearings on research project selection. During the previous 2 days, we have heard testimony regarding the impact of how we select research projects on the kind of research performed and we have examined the methods currently used by several Federal agencies to select projects for funding.

This morning we will complete this survey of Federal agencies with the testimony of Dr. Neville Clarke, director of the Texas Agricultural Experiment Station. Following Dr. Clarke's testimony, we will be privileged to turn our attention to how organizations outside the Federal Government select basic research projects for funding. We welcome Dr. William Brinkman, vice president for research at Sandia National Laboratories, Dr. Maclyn McCarty, vice president and chairman of the science advisory committee of the Helen Hay Whitney Foundation, and Mr. Larry Sumney, president of the Semiconductor Research Corp.

Our final panel will be composed of representatives of IBM, Dr. John Armstrong, and Du Pont, Dr. Richard Quisenberry, who will speak to us of project selection in industry. We're looking forward to the testimony of the several witnesses that will be with us this morning, and we sincerely appreciate their presence here.

At the beginning I would indicate that all of their testimony as submitted to us will be entered into the record, and any other materials that they would like to request be entered, we would certainly be happy to accommodate them.

So, with that, we would like to call Dr. Neville Clarke to be our first witness. Again, Dr. Clarke is director of the Texas Agricultural Experiment Station. Dr. Clarke, we're sincerely grateful that you're here and you may proceed as you wish. You may read your testimony or summarize in any way you would like.

[A biographical sketch of Dr. Clarke follows:]

DR. NEVILLE P. CLARKE

Dr. Neville P. Clarke was appointed Director of the Texas Agricultural Experiment Station on August 1, 1977. The Experiment Station is the State's agricultural research agency involved with work at the main campus of Texas A&M University and outlying research centers and stations across Texas.

Prior to his appointment as Director, Dr. Clarke was Associate Dean for Research and Professor of Physiology at the College of Veterinary Medicine, Texas A&M University. He assumed additional responsibilities as Assistant Director of the Experiment Station in December 1976, managing all animal health research activities of the organization.

In 1981, he was appointed to a three year term as a voting member of the Experiment Station Committee on Policy (the Executive Committee of the national organization representing experiment station directors) and was subsequently elected to Vice-Chairman and Chairman. He is past Chairman of the Association of Southern Regional Directors of Experiment Stations and also chaired the Research Committee of the Great Plains Agricultural Council. In 1985, Dr. Clarke was appointed to the USDA Joint Council on Food and Agricultural Sciences.

Dr. Clarke's professional career began in Air Force Research and Development. He was involved with Aerospace Medical Research directed to safety of both Air Force operational systems and several of the early manned space flights for NASA. For the last four years of his relationship with the Air Force, Dr. Clarke was Director for Research and Development for all of the Air Force's biomedical research activities. Dr. Clarke is the author or co-author of 57 publications and scientific journals, as well as numerous abstracts.

STATEMENT OF DR. NEVILLE P. CLARKE, DIRECTOR, TEXAS AGRICULTURAL EXPERIMENT STATION, TEXAS A&M UNIVERSITY SYSTEM, COLLEGE STATION, TX

Dr. **CLARKE**. Thank you, sir. If I may, I'll just summarize the written comments in a few minutes to try to save you time.

As director of the Texas Agricultural Experiment Station, I have been involved at the national level in planning of agricultural research and working with the U.S. Department of Agriculture in defining our research agenda for the State agricultural experiment stations. Our agency in Texas is the recipient of USDA as well as other kinds of Federal funds for research, and so I speak to you from those kinds of perspectives this morning.

I believe you're principally interested in some comments regarding the peer review system and how it might apply to research in agriculture. We believe we have approached the process of peer review in a variety of ways and we believe that perhaps some of the ways that we go about doing this would tend to provide for some of the concerns that have been expressed by the committee and its assignment. We believe, for instance, that we have a good way of providing for the care and nurture of the young scientist as he's becoming established in our system, and we also have the capability in the system to provide for the continuity of science over time in areas that are critical to agriculture.

To begin with, in the process of setting about talking about evaluating and awarding science resources, we believe that the starting place for this is good, solid research planning and planning that involves the scientist and begins at the grassroots. I've taken the liberty of putting before you several documents that represent first a description of the working methodology in the State agricultural experiment stations. This description defines our partnership with the Federal Government that comes through the Hatch Act, the McIntire-Stennis legislation, and the Evans-Allen legislation. Then it shows you how we've gone about setting the research agenda, beginning at the State level, progressing through regional planning of research in the four regions of the United States, where the State agricultural experiment stations divide themselves, and then planning our research in the State agricultural experiement stations at the national level, and in turn relating this to the research in the Federal agencies that are involved in agricultural research through the Joint Council on Food and Agricultural Sciences that has been mandated by the Congress in the farm bill.

Through that system of planning, we begin with the scientists and work our way through regional and national planning to develop a broad agenda of research that's important at the national level and use this to communicate with the administration and the Congress. By doing so, we believe that we are able to gain a consensus at the scientist level about the things that are achievable and that are important, and through this consensus to be able to better target the grants and interim support that we gain through other kinds of funding.

The State-Federal partnership that I refer to is one in which there is an intentional commingling of State and Federal funds for a common research agenda that addresses regional and national needs. In our system, there are basically three kinds of funding. First are formula-based funds that are essentially awarded to the States after an overall amount has been established by the Congress on the basis of the size of the agriculture, the number of agricultural enterprises, and the like. The second kind of funding that the USDA provides to the States are called special grants—and I'll address those briefly—and then, more recently, we've had added to this a program of competitive grants that's modeled after the NSF approach, and I'll speak briefly about those, if I may.

In formula-based funding, it's important to note that even though the funds are awarded to the State principally on the basis of the size of the agriculture and not the science, each scientist must, in turn, propose a project that is reviewed and peer reviewed at the State and usually interstate level. Those projects are reviewed and approved by the USDA and evaluated on an annual basis, and then have periodic renewals every 3 to 5 years, along with a detailed subject matter or disciplinary review sponsored by the U.S. Department of Agriculture out in the individual experiment stations. Even though these funds are awarded on a formula basis, there is a peer review process at the outset and a process of evaluation throughout the use, and after the use, of these resources. This gives us an opportunity to target the resources that are awarded in these formula-based funds to the needs of the region and to the specific capabilities of the individual States, but still assures that there is a review process. This also gives us an opportunity to provide startup funds for young scientists and to, in some cases, bridge gaps between competitive grant awards in others.

The special grant funds are another kind of funds that are appropriated by the Congress and, generally speaking, represent recognition by the Congress of special problems, as the name indicates, that need to be addressed on a more specific basis. Earlier on, these funds tended to be pretty specifically directed to problems within a State and were, generally speaking, sponsored by the Members of Congress who recognized that problem in a specific locale. More recently, these special grants have been organized and oriented to address regional and national problems, and many of these special grants are now awarded on a competitive basis following peer review. They represent a relatively smaller part of the total program and still give us a method of funding that allows us to focus on emerging problems as well as emerging opportunities.

The competitive grants program began in USDA rather recently. It began with the 1977 farm bill and has grown gradually over the years to the point now where there are something in excess of $50 million in the competitive grants area. This is somewhat different than the other two categories of funding in that the science tends to be relatively basic, and, because in most cases there is not a specific need for coupling to the agricultural research infrastructure in the land-grant institutions, the funds are made available in competition to all of the institutions of higher education, as well as in industry. These are broadly based in terms of their application. They tend to address the sciences that are basic to agriculture. As a matter of history, approximately 60 or 70 percent of these funds have wound up in the land-grant institutions, but a healthy distribution of these funds has gone more broadly into the scientific community.

The dynamics of our system, the changing of our system, is something that would be appropriate to address in the process of describing our total picture. We believe that the research agenda in the State agricultural experiment stations is a dynamic one and one that is subject to a continuing review and redistribution of resources.

At the national level, one of the important things that has happened to allow us to more adequately address these issues is the establishment of the Joint Council for Food and Agricultural Sciences, and the leadership that's been provided in that arena by our first Assistant Secretary for Science and Education in the USDA. This has given us a good bit of focus on science and education within the USDA, and has also provided an important coupling of the Department's research agenda and communication with the rest of the science community in the Federal system.

The State agricultural experiment stations have enthusiastically embraced the competitive grant proposal. We believe this is a very important adjunct to the other methods of funding we have. It allows us to seek out and go for the best quality science wherever it is in the areas related to competitive grants, but we believe it's also important that this is coupled with the continuing healthy and vigorous formula-based funding that allows us to target some of the total resources of the Department of Agriculture into the regionally and geographically specific problems of agriculture. These formula-based funds also provide us the opportunity to have stability and continuity in those areas of our research program which this is important. We believe that even though the process is dynamic, that that stability is an important feature of the total program.

I think it's fair to say that the science community in general would agree that the quality of science in the State agricultural experiment stations is continuing to improve in parallel with the overall changing agenda and approach for science in the United States. We feel today that we represent a unique intersection of opportunity and need in agricultural research. The Nation's agriculture is in a very substantial problem area. Never before, we believe, have we needed an infusion of new technology to make our agricultural enterprises in the United States more competitive and to get them into a posture for enhanced survivability. The opportunity that we have is to exploit some of the new basic biology that is related and applicable to agriculture, and so the need has never been greater and the opportunities for meeting these needs have never been greater because of some of these new capabilities we have in biotechnology.

In conclusion, I might just call to your attention that the State agricultural experiment stations are this year completing their 100th year of research and service to the United States. We plan some celebrations next spring that we hope Members of Congress will participate in here in Washington in March of next year. We believe this is one of the greatest things that the Congress had done in establishing this unique State-Federal partnership for research that we believe has served agriculture well over the years.

I thank you, sir, for the chance to make these comments.

[The prepared statement of Dr. Clarke follows:]

TESTIMONY FOR THE
SCIENCE POLICY TASK FORCE
COMMITTEE ON SCIENCE AND TECHNOLOGY
U. S. HOUSE OF REPRESENTATIVES

STUDY OF GOVERNMENT SCIENCE POLICY

PRESENTED BY

NEVILLE P. CLARKE
DIRECTOR, TEXAS AGRICULTURAL EXPERIMENT STATION

APRIL 10, 1986

THE DYNAMICS OF RESEARCH MANAGEMENT IN THE USDA's COOPERATIVE STATE RESEARCH SERVICE AND THE STATE AGRICULTURAL EXPERIMENT STATIONS

THE DYNAMICS OF RESEARCH MANAGEMENT
IN THE
USDA'S COOPERATIVE STATE RESEARCH SERVICE
AND
THE STATE AGRICULTURAL EXPERIMENT STATIONS

My name is Neville P. Clarke and I am Director of the Texas Agricultural Experiment Station. I am Past Chairman of the Experiment Station Committee on Organization and Policy, which is the national organization representing the State Agricultural Experiment Stations, and current Chairman of that group's Planning Subcommittee. I am Chairman of the National Agricultural Research Committee, a part of the Secretary of Agriculture's Joint Council on Food and Agricultural Sciences, and a member of the Joint Council. My testimony will be presented from my perspective as an administrator of a State Agricultural Experiment Station, as an individual in an interactive role with the United States Department of Agriculture, as a recipient and manager of federal research funds from a variety of sources including the USDA, as an individual involved in committee activities related to national research agenda setting for the State Agricultural Experiment Stations and as one involved in establishing national agricultural research policy.

My testimony will address the following topics:

1) setting the national research agenda;
2) the components of agricultural research funding in the USDA;
3) the unique state-federal partnership;
4) the dynamics of the total research management system;
5) the review and selection of research proposals in the USDA and in the State Agricultural Experiment Stations; and
6) performance evaluation and accountability.

Setting the Research Agenda

The agenda for U. S. agricultural research is driven by the form and substance of agriculture and agribusiness in our country. These are characterized by a geographic and climatic diversity that causes agriculture to be relatively site specific across the country. There are, however, a number of broad, basic issues that are relevant to many regions or states in the country, and it is in these areas of broad-based agricultural commonality that the federal programs of agricultural research are justified. Because of the specific needs of individual states, a system of agricultural research has emerged that involves the State Agricultural Experiment Stations in research missions that address both site-specific and broad regional or national needs.

A major effort at revitalizing research planning in the State Agricultural Experiment Stations has begun in recent years. The planning process for the State Agricultural Experiment Stations has always stressed grassroots planning, looking at the specific needs of individual locations within and between states. But, over the years, a strong program of planning of research at the regional level has emerged for the State Agricultural Experiment Stations. This process involves not only the State Agricultural Experiment Stations but their affiliates that are involved with research in the areas of Forestry, Home Economics and Veterinary Medicine. Also included in the planning activities are the Evans-Allen schools, sometimes called the 1890 Institutions.

With effective planning at the regional level, a national research agenda has been established in the State Agricultural

Experiment Stations under the aegis of the Experiment Station Committee on Organization and Policy. In turn, this statement of agenda by the State Agricultural Experiment Stations is one of the principal inputs to the planning activity of the National Agricultural Research Committee, an entity which is a part of the Joint Council process. The National Agricultural Research Committee provides an opportunity for consensus on research priorities between the State Agricultural Experiment Stations and the federal agricultural research entities including the Agricultural Research Service, the Economic Research Service and the U. S. Forest Service. In addition, it provides an opportunity for input from the non-land-grant elements of higher education involved in agricultural research. The aggregate research agenda of these organizations then becomes the input to the overall Joint Council process in which national priorities for education, research and extension emerge as formal recommendations to the Secretary of Agriculture, in accordance with the 1985 Farm Bill.

Components of Research Funding for the State Agricultural Experiment Stations

State Agricultural Experiment Stations receive sizable appropriations from the state legislatures, generally in accordance with the size and relative importance of agriculture in the individual states. In addition, federal appropriations are awarded by the USDA in three ways: 1) formula-based funding; 2) special grants; and 3) competitive grants. Other federal funds from non-USDA sources may come from any of the federal granting agencies. In addition, the State Agricultural Experiment Stations receive varying amounts of industrial funds as well as private endowments. To a minor degree, they receive some support from selling the excess agricultural products associated with their research.

Funding of agricultural research by the USDA relies on a system of peer review for all three kinds of research grants as well as explicit methods for accountability and evaluation of research productivity. These will be discussed in more detail in succeeding parts of this testimony.

Formula Based Funding

The legal basis for formula funding resides in three key pieces of legislation: the Hatch Act which was passed in 1887; the Evans-Allen program which was first funded in 1967; and the McIntire-Stennis legislation which was passed in 1962. Hatch funds are for a broad scope of agricultural research; Evans-Allen funds support agricultural research in the historically all black institutions; and McIntire-Stennis funds support forestry and forest science research.

Appropriated by Congress, formula-based funds are distributed to the individual states by the Cooperative State Research Service on the basis of a "formula" that, generally speaking, has to do with the number of farms or enterprises, rural population and the size of the agricultural or forest activity in the state. One-fourth of the funds appropriated under the Hatch Act must be spent on cooperative regional research projects.

Though the funds are distributed to the individual states on a formula basis, it is important to recognize that individual projects are subjected to peer review within the individual states. The proposals that are approved at the state level must then be approved by the Cooperative State Research Service before formula funds can be spent on the projects. In the case of regional research projects, a national committee, called the Committee of Nine, reviews and approves regional projects as well as interregional projects that utilize Hatch Act funds.

Individual research projects are evaluated by annual progress reports submitted by the scientists and by periodic reviews. In addition, the Cooperative State Research Service helps the individual State Agricultural Experiment Stations organize and conduct in-depth reviews of a total program or subject matter area approximately once every five years.

Perhaps one of the most important aspects of formula-based funding is the intentional commingling of state and federal funds to support agricultural research. This is a fiscal manifestation of the unique state-federal partnership that has provided the new knowledge necessary to make U. S. agriculture the most productive in the world. This partnership allows a very substantial leveraging of federal funds that are provided for research within the states. The criteria are well established for approving the use of federal formula funds for research that applies to a multi-state objective or to a national problem.

Many studies have been conducted in recent years to measure the so-called "spillover" effects of research that is conducted in one state and benefits neighboring and other states through the transfer of the research results. Studies have documented that for every $1 of benefit acquired through research in a state or region, $4 in benefits spill over into nearby states and regions.

The individual research programs that are partially sponsored by formula-based federal funding are also dynamic in terms of the turnover of research projects, which occurs every three to five years. Although some funding stability is necessary for long-term agricultural research such as the development of new plant and animal germplasm, there is, within the formula-based funding activity, a substantial flexibility of resources, allowing adjustments with redirection of efforts toward critical issues. When these areas of research are viewed in the aggregate from a national perspective, it is sometimes difficult to portray the mobility that actually exists in individual states or in individual projects.

One of the major characteristics of formula-based funding is the utility of these funds in early problem-solving and in developing awareness of emerging agricultural problems and opportunities that need to be addressed by research. There are numerous examples of the timely initiation of new research with formula-based funds; research which has provided the infrastructure and, in some cases, the pilot studies that have identified the need for new intiatives subsequently funded by either special or competitive grants. Changes in the base program of research funded by formula-based resources come not only from internally initiated perceptions of need, but may also be externally driven by interactions with the commodity groups, the Congress and the Administration. There are a number of examples that could be cited to illustrate the dynamics of the base program.

For instance, look at the progress in integrated pest management. Over recent years, there has been a movement away from studies of individual cause and effect relationships associated with plant disease, weeds and insects on the ability to produce agronomic and horticultural crops. As the base program advanced through the decade of the 1970's, interrelationships and total management strategies for these various pests were combined into integrated pest management strategies for individual cropping systems. Now, in the 1980's, pest management strategies are becoming even more sophisticated through the use of artificial intelligence techniques and expert systems. New and more sophisticated capabilities for developing the biologic and economic strategies needed for managing pests in multiple cropping systems have begun to emerge.

Another example is the innovation in beef production. There has been a major shift from earlier research approaches in animal nutrition, genetics and reproduction toward the recognition that a substantially different beef product must be developed to meet current consumer demands and to satisfy diet and health issues. The same general scientific disciplines are now being directed toward the production of a different kind of lean beef product, one that employs not only the traditional scientific techniques but also molecular biology and genetic engineering. This new lean beef product will be produced more economically and will satisfy the purchasing demands of health-conscious consumers.

Special Grants

The USDA's Cooperative State Research Service has awarded special grants over the years for research in areas of need or opportunity targeted by the Congress. Over the years, many of these special grants were highly specific to research needs and problems in individual states. Increasingly, the special grants are funding projects more regional and national in scope and application. Special grants are also now awarded on the basis of a peer review process that ensures the scientific merit of proposals submitted for funding. Special grants, however, are a relatively small part of the total USDA budget for cooperative research with the states. In FY86, $28.6 million was provided to USDA by the Congress for special grants.

Competitive Grants

The 1977 Farm Bill initiated the USDA's competitive grants program and the funding for it has continued to increase in the intervening years. The USDA modeled its program after the National Science Foundation's approach to grant evaluation and award. The State Agricultural Experiment Stations enthusiastically supported this program once initial concern was allayed that funding for the competitive grants program might be transferred from formula-based funding for the base programs of research and to provide new resources for the competitive grants program.

In general, competitive grants have been made available for fundamental biological research that is critical to agriculture. Because of the basic nature of this work, the USDA's policy allows competition from both within and outside the traditional land-grant institutions. Scientific peers are actively involved in all phases of management, including helping to set the research agenda, evaluating proposals, awarding grants and evaluating progress.

The competitive grants program has also been quite dynamic. An early emphasis in plant sciences has broadened to include other areas of biological science. A major initiative in the area of biotechnology was proposed by the Administration and first funded by the Congress in FY85. The biotechnology initiative is still emerging with strong support by the White House Office of Science and Technology Policy and others in the Administration. The competitive grants program has undergone substantial growth; more than $55 million will be awarded in FY86. Thus, almost 20 percent of the USDA funds distributed to the states through the Cooperative State Research Service are now being awarded in the competitive grants program. These grants complement funding provided by other federal research agencies such as NSF, HHS, EPA and the Department of Interior, for broad, fundamental agricultural research.

The Dynamics of the System

As I have indicated, stability is a necessary ingredient in some kinds of agricultural research. But for a number of years, the Administration and the Congress have recognized that the investment in research needs to be evaluated to assure that resources are mobilized to gain the maximum return on that investment. One result was the provision of the 1977 Farm Bill that established the Joint Council on Food and Agricultural Sciences with its mandate for broad communication about science and education in agriculture. But the Joint Council process, in my view, really only became functional and effective under the leadership of our first Assistant Secretary for Science and Education, Dr. Orville Bentley. Under his leadership, the Joint Council has emerged as an effective national forum for the planning and advocacy of science and education in agriculture.

The State Agricultural Experiment Stations have taken a major step forward by setting a national research agenda for their part of the total agricultural research program, as I outlined earlier. And this research agenda is enhancing their interactions with other parts of the agricultural research community.

Overall, there is a very healthy broadening of the communication between USDA and other parts of the federal science and technology community.

Part of the changing picture in agricultural research has been the emergence of the competitive grants program as a well-established part of the total research activity devoted to agriculture; a part now well accepted by the State Agricultural Experiment Stations.

Formula funds continue to have a critical role in maintaining the health and vigor of the so-called base program of research that represents the state-federal partnership. State Agricultural Experiment Station directors are much more comfortable today than in earlier times with so-called "earmarking" of formula funds for specific applications. This has increased credibility and accountability for these funds.

The special grants programs are moving toward a more generally accepted national focus with peer review and selection.

Most critical judges agree that the quality science is continuing to improve in the State Agricultural Experiment Stations. Furthermore, the system has been recognized for its success in balancing the applied sciences needed for effective technology transfer while, at the same time, exploring exciting new opportunities for the development of biotechnology, artificial intelligence, robotics and other new technologies for agriculture.

A unique intersection of need and opportunity exists today in U. S. agriculture. The need exists for new technologies to solve many of agriculture's problems; the opportunity and capability to use the new capabilities in the sciences basic to agriculture to meet these needs exists in the State Agricultural Experiment Stations.

The Hatch Centennial

This year marks the completion of a century of agricultural research by the State Agricultural Experiment Stations. Many people would agree that the Hatch Act of 1887 is one of the truly great achievements of the U. S. Congress and the Federal Government during the last 100 years. It established the State Agricultural Experiment Stations as a part of the total land-grant university network of education, research and extension, an institution that has spawned the most successful food and fiber production system in the world. This tradition of excellence and cooperation at the state and federal level is one that must and will be carried forward into the next 100 years to face the challenges of that timeframe.

I very much appreciate the opportunity to provide this testimony and will be pleased to try to answer any quesions that you may have.

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ESCOP RECOMMENDATIONS FOR FEDERAL FUNDING OF THE STATE AGRICULTURAL EXPERIMENT STATIONS THROUGH CSRS

	FY 1986 Appropriation	Executive FY 1987 Recommendation	ESCOP FY 1987 Recommendation
	------------(*in thousands of dollars*)------------		
RECOMMENDED FUNDING LEVELS FOR CSRS			
Hatch Formula Funds	155,545	155,545	164,308
McIntire-Stennis (Forestry Formula)	12,975	12,975	13,706
1890 and Tuskegee Formula Funds	23,333	23,333	24,648
1890 and Tuskegee Facilities Funds	9,940	9,940	10,000
Animal Health (1433)	5,725	---	6,048
Critical Agricultural Materials	1,195	---	---
Special Grants (P.L. 89-106)	28,632	---	36,666
Competitive Grants (P.L. 89-106)	44,233	42,425	46,320
Federal Administration	1,635	150	150
Rangeland Research, Subtitle M (P.L. 97-98)	497	---	497
Higher Education			
Strengthening Grants-1890 Colleges	1,988	1,988	2,000
Graduate Fellowships	2,982	---	5,000
TOTAL	288,680	246,356	309,343
	(*Increase above FY '86 Appropriation 7.14%*)		
SPECIAL RESEARCH GRANTS			
CONTINUING NATIONAL RESEARCH PROGRAMS			
Integrated Pest Management	3,073	---	3,073
Pesticide Clearance	1,431	---	1,431
Minor Use Animal Drugs	239	---	239
Pesticide Impact Assessment	2,057	---	2,057
Rural Development Centers	379	---	379
Animal Health Research	5,964	---	5,964
Aquaculture Research	298	---	298
Germplasm Resources	994	---	994
Tropical and Subtropical Research	3,231	---	3,231
NEW NATIONAL RESEARCH PROGRAM			
Water Quality and Management	---	---	16,000
SPECIAL PROBLEM GRANTS	10,966	---	3,000
TOTAL	28,632	---	36,666
COMPETITIVE GRANTS			
Plant Sciences	14,413	15,587	16,500
Animal Sciences	4,473	4,473	4,473
Pest Sciences	2,982	---	2,982
Human Nutrition	2,485	2,485	2,485
Biotechnology	19,880	19,880	19,880
TOTAL	44,233	42,425	46,320

DISCUSSION

Mr. **Packard**. Thank you very much, Dr. Clarke.

I didn't realize the Experiment Stations are about to celebrate their 100th birthday. That is quite remarkable. I know, I grew up on a farm in Idaho where the experimental stations were a very integral part of the progress that agriculture was making back when I was young. We appreciate your very good testimony.

I have some questions. I notice in your testimony you mentioned the earmarking of funds, a procedure apparently that is now beginning to be replaced by perhaps more effective processes. Precisely what was the practice of earmarking of funds? Could you give us some examples of how that worked?

Dr. **Clarke**. Earmarking, I'm afraid, is an agricultural term that refers to—I'm sure you know what that means, if you grew up on a farm. Basically, in earlier times, the State agricultural experiment station directors guarded vigorously the unfettered appropriation of these funds without any constraints or without any guidance from the Congress or the USDA. The decisionmaking, in early practice, was made at the State level and that flexibility at the State level was highly valued and still is important to us.

The term "earmarking" now refers to the setting of conditions on the increased appropriations for Hatch funding that the Congress provides. The Congress essentially is now in the era of increasing accountability and being more explicit in their intended use of the funds and has begun to direct that increases in Hatch funds be used for specific things. The Experiment Station directors are pleased to accept that kind of explicit instruction from the Congress and the administratrion and have developed a method of receiving funds for specific purposes and going back to the administration and accounting for the funds to show they were spent in the way that was intended.

An example of earmarking that occurred several years ago was funding for the so-called rural development centers, the several centers across the United States that are directed to some of the sociological problems of the rural communities. New funds were provided for that in the formula-based funds and they're still accounted for in that method.

Mr. **Packard**. You also mention in your testimony, Dr. Clarke, that one of the most important aspects of formula-based funding is the commingling of State and Federal funds. Does that complicate the application process, the fact that you're dealing with both State and Federal funding? Does it require only one approval, or does it require approval at the State and Federal levels?

Dr. **Clarke**. Whether we have separate funds or commingling of funds with these formula-based funds, we have a review at the State level. So that occurs in either case.

In those projects where we propose to use Hatch funding, we forward the projects that have been approved at the State level to the Cooperative State Research Service and they also review those, first looking at the quality and orientation of the science, and second, to see that it is of national significance and not just specific to an individual State. There is a double review process there, but the process has been in place for a number of years and operates quite smoothly and we have no problem with it.

Mr. PACKARD. Does the amount of the Federal participation depend and operate under a set formula in reference to the State amounts, or is that flexible?

Dr. CLARKE. It's quite flexible.

Mr. PACKARD. That depends on each individual case by case?

Dr. CLARKE. Yes, sir. It's important to know that the State appropriations, in those States that have a substantial agricultural industry, very substantially dominate the Federal funding. Another way of looking at that is the Federal funds applied in these formula-based research efforts are leveraged by factors of somewhere between 4 and 10. There is a very substantial total effort that has Federal funding associated with it but is not totally federally funded, not even in some cases dominated by Federal funding.

Mr. PACKARD. Many of our research organizations and researchers have expressed concern about the degree of stability in the Government's funding research programs. Do you find that true also in agricultural research and does the formula funding lend itself to greater stability?

Dr. CLARKE. Exactly. I think, Mr. Packard, that's a very important feature of formula-based funding and of the commingling of these funds, State and Federal funds, in the individual stations.

In earlier history, in reviewing the posture of the State Agricultural Experiment Stations, I believe the science community in general probably felt that, if anything, some of our research was perhaps more stable than it should be, that we were not reacting to the dynamics and need for change as much as we could. We feel now that we have a balanced approach to this, where we are able to provide stability in areas such as in the development of new plant varieties and new animal genetic studies, which tend to be long term in nature, and that we also have the opportunity to have the very rapid turnaround in these funds.

We have, I think, a good record and could show you a number of instances in which problem avoidance and early problem solving has been possible by having funds already available and being able to divert these funds on short notice into areas of immediate urgent priority well before we would be able to subsequently come back and receive broader Federal funding for these areas of concern as new initiatives.

Mr. PACKARD. It would seem that where you have two funding sources—the commingling of State and Federal—and recognizing that the State budgeting process and the Federal budgeting process don't always track with each other—in other words, a State may be healthy in their budget, whereas the Federal Government may be really constrained in theirs—that that would create a problem for agricultural research as you deal with commingling from one State to another with the Federal Government. Does it?

Dr. CLARKE. Well, it really provides, in that sense, a multiple base for fiscal resources. It gives us the opportunity usually to optimize by having more than one source of funding. Being from the State of Texas right now and looking at Gramm-Rudman-Hollings and oil prices and agriculture all at one swoop, I might say that the bottom has hit on both sides of that all at one time for us. But usually there is advantage to having the multiple sources of funding.

Mr. **Packard**. I have other questions but maybe we will submit them to you in consideration of the time. I would ask if the chairman would like to ask questions.

Mr. **Fuqua**. Thank you, Dr. Clarke. We're glad to have you here this morning.

How do you determine the merit of the grants of those funds that come to the university from formula grants? Is that an inhouse review, or is that kind of an executive decision that's made in your office or somebody else's office?

Dr. **Clarke**. We use a combination of things to evaluate these, and I believe my State's operation is probably representative of most of the States that I'm familiar with.

Mr. **Fuqua**. I was going to ask you if that was similar to the others.

Dr. **Clarke**. I believe it is, yes, sir.

The projects originate as proposals by individual scientists. They are reviewed in the disciplinary department by others. They are then reviewed in our State by other parts of our operation outside of that department. After we have them in the shape that we believe we're comfortable with internally, we submit them to at least two other counterparts in other States for review and written comments. We get those comments back and react to those, and then either approve or disapprove the project at the State level. The project is then forwarded on to the Cooperative State Research Service and they make a review in their own judgment regarding the quality of the science. They also make a judgment as to whether that proposal represents an effort that is broader in its application than a single State, which is one of the criteria that should be considered.

Mr. **Fuqua**. It could be eligible for some of the Cooperative State funding?

Dr. **Clarke**. Yes, sir.

Mr. **Fuqua**. But if it just had an application probably to that one State, then it would not trigger that.

Dr. **Clarke**. That's right. We would tend not to submit those to CSRS for approval, and if we submitted some they thought were not appropriate in that context, they would reject them.

Mr. **Fuqua**. Have you been satisfied with the Competitive Grant Program that USDA has initiated in the last 10 years?

Dr. **Clarke**. Yes, sir. We are very excited about the Competitive Grants Program. We believe that it offers a good opportunity to take advantage of some of the scientific opportunities that are very important to agriculture.

In the more basic sciences, we have, as does the science community in general, a good opportunity to let that science come and go in terms of the quality of the science. Although stability is important there, we can, through competitive approaches, we believe, make sure that we're getting that science put in the right place where the ability to respond will exist.

I think, as I have said earlier, that it is important that those resources can be competed for by the non-Land Grant community as well as by the Land Grant community, so we can truly take those moneys where the science is.

Mr. FUQUA. One of the questions that has come up in the course of these hearings has been the opportunity of interdisciplinary work. Do you find the present situation, whether it be the formula or the cooperative or competitive, that that is encouraged or discouraged?

Dr. CLARKE. I believe that scientists, at least in our community, recognize increasingly with each year that passes that they can achieve more as individual scientists in collaborative efforts than they can as individual scientists, so there's a substantial motivation from the grassroots to move in that direction.

In our State, we have created resources that are specifically targeted for collaborative and interdisciplinary research, creating incentives for scientists to communicate with each other. We find that we can initiate teams of people working together through the initial carrot dangling, and that once those people begin to have the satisfaction of working together and seeing the achievement, we can pool those resources and direct them in other places and that the teams do last.

Certainly in the planning documents we have placed before you here we stress very strongly the importance of multidisciplinary research to achieve the solution of problems rather than strictly disciplinary research, per se.

We see in our individual States a matrix management approach, where problem-solving research is one dimension of the matrix, and the other dimension is the quality of science within individual disciplines.

Mr. FUQUA. It would seem to me that no other discipline probably more than agriculture could benefit from interdisciplinary research.

Dr. CLARKE. I certainly agree.

Mr. FUQUA. And could contribute a lot to other disciplines.

Thank you, Mr. Chairman.

Mr. PACKARD. Thank you.

Mr. Brown.

Mr. BROWN. Thank you, Mr. Chairman.

Basically, research in agriculture, as in any other field, depends upon achieving an adequate supply of high-quality research personnel who can operate in an environment that's conducive to their best activities.

Do you feel that the system that we have in agriculture is one that attracts these kind of people into agricultural research, or do we need to do something to jazz the field up a little bit to do a better job of attracting such people?

Dr. CLARKE. I believe we're a little better off than we were a few years ago in that respect, Mr. Brown. I believe, for instance, in the area of plant molecular biology, we have begun to attract very qualified people into the fields of agriculture for that kind of science, partly because there are beginning to be resources available to carry out that kind of research, and partly because, I believe, the young scientists who are interested in that kind of research find some advantage in recognizing that if they conduct research in a broadly based Land Grant institution in some of the basic sciences, there will be an existing infrastructure of more applied science that will help them target the research that they do and take the products from the biochemistry lab back out to the field.

So there is a perception of the broad infrastructure that exists being supportive to some of the more basic sciences that are going to be so important to us in the future.

Mr. BROWN. Well, there was a common impression about agriculture research for many years, that it was getting kind of stodgy, that there weren't these fascinating, attractive, demanding research opportunities. And that can happen when you get into an institutional setting where money keeps flowing in every year and there's no particular challenge that demands that you do something exceptional. That's been one of the problems with formula grant research, but it may not be the formula so much as it is the conception that maybe agriculture was not as exciting a field as, say, molecular biology and biomedical research.

Dr. CLARKE. Exactly. I believe that impression is still with us to some extent. I think most people believe that that's less of a problem today than it was 5 years ago, that we're moving not only in just the area of basic plant and animal molecular biology, but also in the application of computer science in the use of artificial intelligence for the development of decision aids, the areas of robotics. There are a variety of contemporary, cutting-edge, and highly interesting sciences that can now be applied to the problems in agriculture. I believe that as we are able to do that with the kind of resources that it takes to get the job done, that it will attract the people.

Another thing I know you've had substantial interest in in recent years is whether or not we're doing the kind of training of these scientists within the Land Grant community that will give them the background and awareness that will let them perform. That's a continuing concern and certainly an active part of our agenda for the future.

Mr. PACKARD. Would the gentleman yield on that? That's an intriguing question.

Mr. BROWN. Certainly.

Mr. PACKARD. Are you finding that more of your agricultural researchers and scientists are coming out of the agricultural community, or are they being transferred out of the academic community?

Dr. CLARKE. Well, I believe in the area of, say, molecular biology and related basic sciences, the people who are performing in leadership roles in agriculture today represent a community that, in very substantial measure, came from other parts of science. They came from training in medical sciences, from very basic sciences across the entire community, and have targeted some of the interest into areas that are going to ultimately be related to agriculture.

Some of the people who are in these areas have been trained within this community in the earlier parts of their career and have gone elsewhere to obtain graduate training and come back into agriculture. So there is kind of a diversity there. But we believe that as we move along and gain those kinds of people into the community, we will be able to train new scientists that will have a good solid foot on the sciences and also a perspective for the application of their science into agriculture.

Mr. **Packard**. I appreciate the gentleman yielding.

Mr. **Brown**. It appeared yesterday, from testimony we had from a representative of NIH, that there is no perfect system, that those in NIH who have used the competitive grant system over the years have to continually critically analyze the process and see if it's working. They have come to the conclusion that they needed to do something about this continued demand for proposal writing by giving longer term—5- and 10-year grants.

Of course, you have that kind of stability in agricultural research, and you have a need to continually critically analyze what you're doing to see that you're on the cutting edge of new opportunities and that sort of thing, and you, as you have already indicated, welcome some infusion of the competitive grant process in order to provide that kind of opinion.

It's rather interesting, that almost any system can work if it's subject to continued critical analysis in the light of whether it's achieving its fundamental goal, and that is something that is very difficult to write into a system of any sort.

There is one further question that relates to this, and that is the question of the adequacy of the formula itself for the distribution of funds. As you know better than I, probably, the Congress about every 25 years or so has had to reexamine the formula and determine if it's providing an equitable distribution of funds in the light of the problems of this generation and the relative changes in the States and the role of agriculture and so on.

Do you think we need to take another look at the formula process itself and see if it might need modification?

Dr. **Clarke**. I'm really not sure I can quantitatively address that on the basis of my personal knowledge. I'm aware that, in preparing for the last farm bill, that some thought was given to this.

Mr. **Brown**. If I may interrupt you at that point, there was a study done through the extension operators. There were certain recommendations made. The current status is that the farm bill contains language asking the Department to do a 1-year study and make a recommendation.

Dr. **Clarke**. I think the study is certainly indicated. In looking very generally and very broadly at that, we are finding that still the formula is directing money to the States who have the largest agriculture. The interesting sorts of things are how it's driven by the number of farms rather than the total size of agriculture, and that's a shifting issue, that the family farm situation leads you into this kind of discussion. I think it bears careful study.

Mr. **Brown**. For the benefit of my colleagues, the formula has been a good formula. It has worked well to distribute money. But it is based upon factors that give heavy weighting to the rural population and the number of farms, rather than on the volume of agricultural activity, per se. As the nature of farming changes in this country, that tends to penalize the States where they conduct a large volume of high technology, large-scale agriculture in favor of the smaller States with large rural populations, but not necessarily producing so much in the way of farm commodities. Naturally, that sets up a certain tension in the system, mainly involving California and Mississippi.

Mr. **Packard**. And Florida.

Mr. **Fuqua**. We're changing our view.

Mr. **Brown**. I see.

I have no further questions.

Mr. **Packard**. Thank you, Mr. Brown.

There may be other questions, again, but we will go to the other panel simply because of the time this morning. We have a significant number of witnesses. But if any of the members wish to submit questions, would you be willing to answer them in writing?

Dr. **Clarke**. Of course.

Mr. **Packard**. We appreciate very much your being here, Dr. Clarke, and your excellent testimony.

Dr. **Clarke**. Thank you.

PANEL 1

Mr. **Packard**. We will be pleased now to move to Panel No. 1, with Dr. William Brinkman and Dr. Maclyn McCarty and Mr. Larry Sumney. We will hear from them in that order. Mr. Brinkman will speak to us first. He is the vice president for research at Sandia National Laboratory in Albuquerque, NM.

We are grateful to have you with us, Dr. Brinkman. You may proceed. We would appreciate it if you could hold your testimony to approximately 5 minutes in each case, if that's doable, and that will allow us time for questions. Thank you very much.

STATEMENT OF DR. WILLIAM F. BRINKMAN, VICE PRESIDENT FOR RESEARCH, SANDIA NATIONAL LABORATORIES, ALBUQUERQUE, NM

Dr. **Brinkman**. Good morning. Thank you, Congressman Packard, Congressman Brown.

I am actually here for two purposes today. One is the fact that we're now announcing a new physics survey from the National Academy of Sciences and I wanted to make sure I came over and presented these things.

Mr. **Brown**. Did you bring an extra copy for me?

Dr. **Brinkman**. I brought a summary. This is a very short summary, a report that I think you will want to pay some attention to in the next year. There are lots of extra copies here.

Mr. **Packard**. Would you like to have any portion of this submitted for the record?

Dr. **Brinkman**. I think the summary would be very nice.

Mr. **Packard**. I would hate to see us have to print the entire report.

If you would like to proceed, then, Dr. Brinkman.

Dr. **Brinkman**. What I'm here today to talk about is how research is managed in a somewhat different environment in our universities and some of our national laboratories. In the universities, of course, research is primarily managed and selected by the peer review process, and that's based on the fact that universities have very diverse faculties and some technical evaluation has to be made. That is done through the National Science Foundation and other institutions.

In some of our national laboratories that are devoted to basic research, they usually have a large facility that they're running and the research they do in-house is directed toward enhancing and fostering that facility and getting the best possible research out of it.

In the case of Sandia, it is quite a different case. It is more like places like Bell Laboratories, IBM, and other industrial institutions, where, in fact, we have a national laboratory that has a mission that is outside of research. The mission of Sandia is a primary mission of nuclear ordnance and, therefore, research is viewed as something that has to be part of that mission. The way you do research under this environment is you chose the fields of research that you believe will have long-term relevance to the mission of the company. That makes research a very integral part of the whole mission of the company and the company itself. By doing that, by having that kind of research, what you do is you pick those kinds of fields that you think will have that.

For instance, we have a fairly large effort in condensed matter physics. We have an effort in materials science, geoscience areas that we think are relevant to the long-term mission of the laboratory. These things must then be nurtured along and kept a part of the total effort. They cannot become too "Ivory Tower" because they will become irrelevant to the rest of the company and sooner or later somebody will cut them. They can't be too applied because then you can't get good research people. So there's a very delicate balance that must be obtained in that sort of thing.

Another thing that you have to establish is a very continuous spectrum. You have to have scientists out there competing in the world of both fundamental science and then you have to have next to them somewhat more applied and then on to the development area. They must maintain that spectrum so that there is a communication between your research and your development organizations.

The other kinds of things you do under these circumstances is, after having selected those fields that you think are most likely to be relevant to your long-term mission, you have got to go out and hire the best scientists that you can find in those areas. I think that's really the fundamental thing that you do when you manage such research.

You have to have the steady funding base. In our particular case, that's one of the benefits of the Office of Military Applications in the Department of Energy. We have had a steady funding of tech-based in our weapons laboratories and it has allowed us to fund research in a steady consistent fashion.

The other thing you then do under these circumstances is look for new concepts and new ideas in your research organization, and you let these grow in a fairly natural, technology-driven way, as long as it's consistent with the overall mission of the laboratory. And you can do that a number of ways. Basically what you often do is allow your scientists, who are the most knowledgeable people in your organization about different pieces of science, to look at these things and decide whether they think it's worth working on. By these kinds of internal peer reviews, you end up with projects that grow and become bigger. Eventually, of course, as they move into development, you must formalize that procedure and organize real development projects.

Finally, I think the final ingredient in this kind of environment is to make sure that your management is chosen from the best scientists you have on your staff because, after all, you're asking management to make a real judgment of what's good science and what's bad. If you don't do that, you miss out and, generally, research organizations do not work well.

Sandia has not only the work of nuclear ordnance, but its secondary mission is energy-related projects. We have a large program in solar, fossil, wind research, and nuclear waste management—things like this. We regard our Basic Energy Sciences Program at Sandia as basically the tech base on a research part of that entire effort. Consequently, once again, we try to establish this continuous spectrum from research to more applied things. Frankly, the way we manage that basic energy sciences effort at Sandia is to select the types of proposals we sent in to BES.

If you really asked me how I would prefer doing that, I would prefer having a situation in which we manage the entire spectrum of things from a laboratory point of view.

Another area which we have gotten into more is working for the Department of Defense—advanced conventional munitions and things like that. There, the way we have established research is to have a tax on all defense work so that we keep our research tech base going, so that we can manage the same kind of mix that we've had in the weapons program.

I think you can think about this model in different environments and larger scales if you want to, and I think even with the Nation you could ask yourself how your spectrum of applied research and development looks and see if it has a balance. From the physics survey point of view, I think that we would maintain that, when you look at it from that picture, the balance has gotten so that research is probably not funded with as big a fraction of the total R&D effort as we would like to see.

In any case, I hope that gives you some idea of how we do things at Sandia. Thank you.

[The prepared statement of Dr. Brinkman follows:]

Testimony
to
House Committee on Science and Technology
by
William F. Brinkman
Sandia National Laboratories
April 10, 1986

Chairman Fuqua and honorable members of the Committee on Science and Technology. Thank you for the opportunity to speak to you regarding Research Project Selection and more generally, Research Management. Since the latter is my full-time occupation, I am particularly fond of the subject.

Research is managed in a variety of ways depending on the institution at which it is performed and on the goals and missions of the institution. Our research universities have as a primary mission the training of students at the Ph.D. level. In order to do this they employ a faculty of experts in basic research who can direct students in forefront research projects. This country has traditionally, and I believe correctly, had the policy that research at universities should be at the cutting edge so that students learn not only how to perform research but also how to select problems that are worth solving. Because the universities have a diverse faculty, it is difficult for them to individually judge the quality of research of their staff and consequently we have developed an elaborate peer review process for them. It is not a perfect system but it does introduce a reasonable technical evaluation which I regard as essential to research management.

In other settings, managing research is done quite differently. In those national laboratories where basic research in a particular field of science is one of their missions, they generally manage the large facilities that are necessary for advancing that field. The in-house research is usually directed toward research that is commensurate with the exploitation of this facility. Such research usually leads to a much more efficient utilization of the facility and a high-quality, in-house program means a better selection of research programs for the facility.

Today, I would like to describe how basic research is performed and managed in a different environment which only occurs at certain industrial laboratories, at Sandia National Laboratories, and perhaps at a few other institutions throughout the country. This is research that is part of a much larger institution whose primary goals or missions are product oriented. For example, Sandia National Laboratories has as a primary mission the development of nuclear weapons ordinance; AT&T Bell Laboratories has a primary mission of developing communications equipment for AT&T; and IBM has as their primary mission the development and sales of computing equipment. In these environments, research is performed for the purpose of developing the technical and scientific basis for future technologies relevant to the institutions missions. It is an integral part of the institutions total effort. Given this general principle, exactly how it is managed follows. First, the fields of research that are emphasized must be commensurate with this stated goal. For example, Condensed Matter Physics and Materials Science are very important to the electronic components development. Indeed, these fields have been the driving force of that development and all three of the institutions mentioned maintain strong efforts in these fields.

- Having selected the fields of research, we also find that it is necessary to ensure that there is a continuous spectrum of activities from the basic research to more applied research to development so that the research results are assimulated into the company and at the same time are stimulated by the needs of the development process. This, of course, does not mean that any individual spans this entire spectrum but rather that the sum of the activities in research and development do so. Thus individual researchers are allowed great lattitude in pursuing the best research and competing with the worlds top scientists, but associated with them, are people who are interested in somewhat more applied research and still others who are further into development.

Creating a successful research organization in these environments is thus a delicate balance. If the research becomes too "Ivory Tower" it becomes disjoint from the rest of the institution and eventually is regarded as useless (and usually cut). If it is forced to be too applied, one cannot attract and keep the quality research staff that is desirable. There are many other essential ingredients in managing research in such an environment:

- Hire and support the brightest and highest quality people. A research institution is only as good as its staff. After all, ideas and concepts are had by individuals and the quality of the staff creates the environment that stimulates them. In addition, as a peer group, the staff itself generates the pressure for the individuals to strive for a high level of performance.

- Steady funding levels. The research scientists, having been carefully selected, must be well supported so that he can concentrate on research and not spend time justifying the next piece of equipment he needs.

- Allow new concepts to grow at a technology driven pace within the broadly defined mission of the Laboratory. Important new directions start as a concept or discovery--usually by a small group or individual. The growth and development of the idea must be nurtured at a level that is determined by its relative importance. One way this is done, is to allow the research staff itself to decide whether they wish to pick up on the idea. After all, who else can be a better judge than the scientist who understand the science and the Laboratory mission and whose decision will determine the ultimate fate of his career.

Finally, I should like to point out that an absolutely essential feature of these research institutions is that the management be chosen from the best members of the research staff. Managers must be technically competent and have good judgment of scientific capability. That is, they must recognize good science.

Only a few institutions have been able to successfully manage this type of research over a long period. Usually, this is because of the lack of steady funding and of the recognition of the importance of long-term research to the institution as a whole. At Sandia, the commitment has been possible because of the "level of effort" funding provided by the Office of Military Application of the Department of Energy. In recent years, we have established a sizeable energy program dealing with utilization of solar, fossil, and nuclear energies, and with problems of waste management. We regard the Basic Energy Sciences funding as that which funds our research that is coupled to these energy programs. It thus is an essential part of the integrated effort of the Laboratory as a whole. We currently manage this by controlling the proposals submitted but would prefer more block funding.

I would suggest that this model of managing research might be thought of in broader terms for the United States as a whole. One could consider how our research feeds into the applied research and then into development and examine the mix of research to ascertain whether it fits the needs of the country. One would need to take into account the variety of driving forces and recognize the importance of man's quest for basic knowledge in a broad sense. I believe we would find that basic research has been deemphasized since the sixties relative to our total R&D effort and that while it has been increasingly supported by the Reagan administration and Congress in recent times, it has still not fully recovered.

Mr. **Packard**. Thank you, Dr. Brinkman.

We will next hear from Dr. Maclyn McCarty, who is the vice president and chairman of the Science Advisory Committee, Helen Hay Whitney Foundation, from New York City. We appreciate your being here, Dr. McCarty.

[A biographical sketch of Dr. McCarty follows:]

Dr. Maclyn McCarty

Dr. Maclyn McCarty, professor emeritus of The Rockefeller University, is a world-renowned leader in research on the transformation of pneumococcal types, the biology and immunochemistry of streptococci, and rheumatic fever.

A member of the University faculty since 1941, Dr. McCarty was associated with Oswald T. Avery and Colin MacLeod in the landmark experiments which led to the demonstration, in 1944, that deoxyribonucleic acid (DNA), a major component of chromosomes, is the substance that transmits hereditary information.

The significance of the work of Dr. McCarty and his collaborators lay in the fact that transformation—the artificial transfer of genetic material from one bacterium to another—was brought about in a test tube by a highly refined substance consisting principally of nucleic acid. This was the first direct evidence that DNA, a substance previously thought to lack the necessary chemical diversity, is responsible for genetic continuity. A corollary of this discovery that nucleic acid in some way specifically controls the synthesis by the cell of certain products has had important and widely ramifying implications.

Dr. McCarty's scientific memoir, *The Transforming Principle: Discovering that Genes Are Made of DNA*, was published by W.W. Norton & Company in 1985.

Dr. McCarty's studies on streptococci include the isolation of both extracellular and cellular components of the microorganism. In particular, he has isolated the external protective coating, the cell wall, and studied its several components with regard to their chemical structure, biological properties, and relationship to the production of disease.

Dr. McCarty was born in South Bend, Indiana, on June 9, 1911. He was graduated from Stanford University in 1933 and The Johns Hopkins University in 1937 with the degree of M.D. At Johns Hopkins, he was successively intern, assistant resident, and assistant in the Department of Pediatrics. He was a medical fellow, doing research on sulfonamide drugs at New York University from 1940 to 1941, before he joined what was then known as The Rockefeller Institute for Medical Research as a National Research Council Fellow. From 1942 to 1946 he was on active duty as a lieutenant commander in the United States Navy Medical Corps with the Naval Medical Research Unit based at The Rockefeller Institute Hospital.

He was named an associate in 1946, associate member in 1948, member in 1950, and professor and senior physician in 1958. From 1960 to 1974, he was physician-in-chief of The Rockefeller University Hospital with responsibility for the development of clinical research and supervision of clinical care.

He became a vice president of the University in 1965 and was named a John D. Rockefeller, Jr. Professor in 1977. He retired as vice president in 1978, but held the Rockefeller chair until 1981, when he became professor emeritus.

Dr. McCarty has received many honors for his scientific contributions. In 1963 he was elected a member of the National Academy of Sciences, and in 1946 he received the Eli Lilly Award in Bacteriology and Immunology. He is a member of Phi Beta Kappa, Alpha Omega Alpha, and Sigma Xi. In 1975, he was elected to membership in The John Hopkins Society of Scholars. He received an honorary doctor of science degree from Columbia University in 1976, from The University of Florida in 1977, from The Rockefeller University in 1982, and from The Medical College of Ohio in 1985.

In 1977 he received the first annual Waterford Biomedical Science Award of Scripps Clinic and Research Foundation. He also received the Academy Medal of the New York Academy of Medicine in 1979, the 1980 Medal of the Board of Regents of the American College of Physicians, and the 1981 Robert Koch Medal of the Robert Koch Foundation.

In 1982 he was awarded the Order of the Republic in the First Degree by the Egyptian government for his service on the advisory board of the Biomedical Research Center for Infectious Diseases of the Ministry of Health, Egypt. In 1984 he was awarded the Commander's Cross of the Order of Merit of the Federal Republic of Germany, and the same year he was among the first group of five recipients of the *Who's Who in America* Achievement Awards.

Dr. McCarty is chairman of the scientific advisory committee of The Helen Hay Whitney Foundation and was chairman of the Health Research Council of the City of New York from 1972 until it was dissolved in 1975. He was a member of the Council of the National Academy of Sciences from 1973 to 1976, and is a member of the Academy's Institute of Medicine. He is Chairman of the Board of Directors of the Public Health Research Institute of the City of New York.

He is an editor of *The Journal of Experimental Medicine* and was a member of the editorial board of the *Proceedings of the National Academy of Sciences* from 1972 to 1977.

He is also a member of the American Academy of Arts and Sciences, American Association for the Advancement of Science, American Association of Immunologists, American Pediatric Society, American Philosophical Society, American Society for Clinical Investigation, American Society for Microbiology, Association of American Physicians, The Harvey Society (president 1971-1972, secretary 1947-1950), Infectious Diseases Society of America, Interurban Clinical Club, New York Academy of Medicines, New York Heart Association (president 1969-1971), Peripatetic Club, Society for Experimental Biology and Medicine (president 1974-1976), and Transplantation Society, as well as honorary member of the Deutsche Gesellschaft für Hygiene und Mikrobiologie.

Dr. McCarty was a trustee of Associated Universities, Inc. (1977-1980) and served on the scientific advisory committee of the Cornelius Vanderbuilt Whitney Laboratory for Experimental and Marine Biology, University of Florida (1977-1983).

He resides with his wife, the former Marjorie Fried, in New York City.

STATEMENT OF DR. MACLYN McCARTY, PROFESSOR EMERITUS, THE ROCKEFELLER UNIVERSITY, NEW YORK, NY, AND VICE PRESIDENT AND CHAIRMAN OF THE SCIENCE ADVISORY COMMITTEE, HELEN HAY WHITNEY FOUNDATION, NEW YORK, NY

Dr. McCarty. Thank you.

The Helen Hay Whitney Foundation is a small to medium-sized foundation, private foundation, that supports biomedical research through the operation of a postdoctoral fellowship program, with all of its available funds going into this program. It is an operation that manages, I think, with minimal administrative costs. There is only one full-time employee in the operation.

The fellowship program was chosen initially as a way by which the foundation, a foundation of this size, could make significant contributions to biomedical research. It does this in essentially all the major fields of biomedical research.

I think it is generally considered that this early postdoctoral period is an important junction in the training of all young investigators in preparing for their future careers.

We have had this program in operation for about 30 years. Over that period of time we have awarded some 550 3-year fellowships. The program is operated by a Scientific Advisory Committee which has a rather stable membership. In the past 10 years, for example, we have operated with the same six members, all of them relatively senior scientists and members of the National Academy of Sciences. The group has worked together closely and we have found it useful to have a continued and stable membership of this kind.

We get something on the order of 250 applications annually for about 19 to 20 3-year fellowship awards that we can make in each year. The application for one of these awards is a rather extensive one. It covers the various aspects of the previous training and research of the application, background, the full proposal for his proposed research, letters from three recommenders and so forth. This is an annual round of applications with an August 15 deadline, and I, as Chairman of the Scientific Advisory Committee, screen these applications at the outset, sorting them into different categories, as well as eliminating those that are obviously not competitive or are out of policy for one reason or another. The remainder are then distributed to the other members of the Scientific Advisory Committee who review their group of applications in detail, and each of them selects those that they feel should be interviewed—a personal interview. We usually have some 50 to 60 of the group that are finalists and, therefore, interviewed.

This is something that is relatively unusual in the current selection of postdoctoral fellows, that of a personal interview. We have been doing it since the beginning of the program and have reviewed from time to time whether this additional effort and expense is worth it. Our general conclusion has been, on each occasion that we've looked at this, that the interview does add something—difficult to define, perhaps—but it adds something to the decisionmaking process in trying to select fellows from a very good group that we have in this 50 to 60 finalists each year.

After the interview process, which goes on in the fall, the committee meets and has a day-and-a-half of discussion and review of these applications, with the assignment of priority scores and the selection of the fellows pretty much on the basis of their ranking and priority rating, with some attention to such things as field. We would not, for example, select most of the people from one single field. We would tend to diversity if we have, as we usually do, a large group of excellent applicants who fall below our cut-off point, the cutoff point being determined on the available funds as to how many fellows we can support.

The idea is then to select the most promising of these people from the point of view of academic excellence and research productivity, and we also take into consideration the nature of their research project and the supervisor and location where they will get their research training.

I think I can say that over the 30 years we have felt the system has worked and, therefore, while we've modified details of it, it has been a selection system that seems to have given us a high success rate in terms of the performance of the individuals. About 90 percent of the total of the 550-odd are still engaged in research, and the program itself I think is considered to be one of the prestigious awards among the young postdoctoral fellows. It has maintained this now for the past 20 years.

I think that gives the picture of this particular kind of postdoctoral fellowship program which, as I say, we consider an important way to support biomedical science at this juncture of the training of the individuals and one in which a foundation of our size can cope with with some success.

[The prepared statement of Dr. McCarty follows:]

THE HELEN HAY WHITNEY FOUNDATION

Postdoctoral Fellowship Program in the Biomedical Sciences

Background

The Helen Hay Whitney Foundation is a medium-sized private foundation that was established in 1947 by Joan Whitney Payson in memory of her mother. While the activities of the foundation were initially focused on the support of research on a specific disease entity (rheumatic fever and rheumatic heart disease), the importance of basic biomedical research was emphasized from the beginning. In 1956 it was decided to broaden the mission of the foundation and to devote its entire resources to the support of postdoctoral fellowships in the basic biomedical sciences. This program has now been active for about 30 years.

The fellowship program was established by Dr. Walter Bauer of Harvard Medical School and the Massachusetts General Hospital, who served the foundation as Vice President and first Chairman of the Scientific Advisory Committee. Following his death in 1962, he was succeeded in these positions by the undersigned.

Operation of the Program

The operation of the fellowship program is delegated by the Board of Trustees to a Scientific Advisory Committee composed of established scientists in a variety of fields of biomedicine. The members of the committee do not have fixed terms, and the pattern of membership has been characterized by sustained associations that have proved of great value in the administration of the program. At present, two of the five members originally appointed in 1956 still serve on the committee, two were appointed in 1964, and one in 1977. An additional member was appointed by the Board in January 1986.

The fellowship program is run with a minimum of overhead expense so that the bulk of the foundation's income can go directly to the support of postdoctoral fellows. There is only one full-time employee of the foundation, the Administrative Director, who manages the day-to-day operation of the program with the aid of part-time assistance as required. She works in close association with the Chairman of the Scientific Advisory Committee. The principal programmatic expense, other than fellowship stipends and funds for laboratory support and travel, is an annual meeting at which all of the current fellows are brought together for a scientific session. The fellows in their final year present papers on their research accomplishments, with adequate time for informal discussion and interchange.

The immediate postdoctoral period is an important part of the training of young scientists, and we believe that a foundation of this size can make a significant contribution in this area. We are encouraged to continue our program because of its apparent success as indicated by the prestige that these awards have acquired in the biomedical community. In the almost thirty years of operation of the program, we have been able to award over 550 three year fellowships. The vast majority of these fellows (85-90%) are still active in research or research administration, and a large number have senior positions at their institutions.

Selection of Fellows

The foundation has an annual round of fellowship selection with an application deadline of August 15. Now that the program is well known, we rely on published announcements in Science rather than attempting to circularize all of the relevant institutions. Currently, we receive about 250 applications each year. Since we have funds for only 19-20 new three-year awards each year, these awards are highly competitive.

Our fellowships are designed for beginning postdoctoral training, usually within one year of the receipt of the doctoral degree. U.S. citizenship is not required, but applications from abroad are accepted only from candidates seeking training in this country who will be available for personal interview during the review process. The application forms provide for a variety of information on the applicant's background, education, and previous research accomplishments, as well as submission of a biographical sketch and a full description of the proposed research to be conducted under the fellowship. Reference letters from three former teachers or supervisors are requested. The applicant selects his institution and supervisor for postdoctoral training and arranges for the prospective supervisor to submit a letter endorsing the application and indicating acceptance of the terms of the fellowship if awarded.

The review process includes the following steps:

1. After the applications are processed, they are screened by the Chairman of the S.A.C. All are eliminated that are inadequate or not within policy (e.g., too many years of previous postdoctoral experience).

2. The remaining applications are divided into disciplinary categories and forwarded to the other members of the S.A.C. for secondary review. Each member indicates those applicants who should be scheduled for personal interview as finalists. 50-60 of the most promising applicants (or roughly 20-25% of the total) are selected in this way.

3. Personal interviews are assigned to the members by the chairman, each candidate being interviewed by one member, and the interviews are carried out in October and early November.

4. The completed applications of the finalists, including the interview reports, are then reviewed by each member, who makes his assessment and assigns a priority score.

5. The committee meets in December and spends one and one-half days in making the final selection of the 19-20 awardees. Each applicant is discussed after a presentation by the interviewer and an average priority score is determined. This score is the principal criterion for selection and the top 19-20 are generally recommended for awards, although other factors may be considered on occasion. 3-5 alternates are also designated in the event that the award is declined by some of those selected in the first group.

6. The successful candidates are notified by mid December that they they have been recommended to the Board of Trustees for awards. Official appointments are made by the Board at its January meeting.

The object of this process is to select the most promising young biomedical scientists. While personal characteristics, such as academic excellence and research productivity, are the most important criteria, other factors enter into consideration. These include the setting for the proposed research and the qualifications of the proposed supervisor. The aim is to provide a research environment in which an able young scientist can develop his or her full potential.

Maclyn McCarty

Maclyn McCarty, M.D.
Vice President and Chairman,
Scientific Advisory Committee
The Helen Hay Whitney Foundation

Mr. **Packard**. Thank you very much, Dr. McCarty. I appreciate your testimony.

Mr. Larry Sumney is president of the Semiconductor Research Corp. from North Carolina. We appreciate your being here. You will be our next witness and we'll proceed with your testimony, Mr. Sumney, before we have questions for the panel.

[A biographical sketch of Mr. Sumney follows:]

Larry W. Sumney

Mr. Larry W. Sumney was born in Washington, Pennsylvania. He received his B.A. degree from Washington and Jefferson College in 1962 with Honors in physics. He received his Master's degree from George Washington University in 1969 and completed his course work toward the DSc degree in Systems Engineering in 1973.

From 1962 to 1967, he was employed as a research physicist by the Naval Research Laboratory (NRL) where he performed basic research into properties of silicon oxides, thin film device structures, and microelectronic processing. From 1967 to 1972 he was employed as an electronics engineer at NRL and was responsible for the innovative application of digital microelectronics in advanced IFF, navigation, and communication systems. In 1970, Mr. Sumney was named a NRL Edison Scholar for two years. During this period, he completed his course work toward the D.Sc. degree.

In 1972, he joined the Naval Electronics Systems Command and became the manager of the Tri-Service Charge Coupled Device (CCD) Program which was managed by the Navy. From 1972 to 1975 his management responsibilities increased to include technical management of a broad spectrum of solid state and electro-optical technology programs. In 1975, he was named Head, Solid State and Special Device Technology Branch. In 1977, Mr. Sumney was additionally assigned the duties and responsibilities of Research Director for the Command.

In August 1978, Mr. Sumney was assigned, under a special Navy personnel assignment, to the Office of the Under Secretary of Defense for Research and Engineering (OUSDRE) as a Staff Specialist for Electron Devices and Integrated Circuit Technology. In this assignment, Mr. Sumney conceived a new initiative in integrated circuits for which he established advocacy in the three services as well as in OUSDRE. The planning cycle for this initiative officially began in October 1978. The new initiative was titled the Very High Speed Integrated Circuits (VHSIC) Program. Mr. Sumney was instrumental in the early planning and organization of this high visibility program and played the key role in addressing the management concerns of the Congressional Committees prior to the Program's authorization and appropriation. He was successful in melding the technical, managerial, and contractual concerns of the three Services into a coherent technology initiative to be managed directly by OUSDRE. As a result of his efforts, the FY-80 VHSIC Program officially began. The program has become the largest technology program ever undertaken by the D.o.D. and has served as a model for OUSDRE—managed Tri-Service technology efforts.

In October 1980, Mr. Sumney joined the Office of the Under Secretary of Defense for Research and Engineering staff and was formally named as the Director of the VHSIC Program Office. He was also a Staff Specialist for Electron Devices and Integrated Circuits Technology. Mr. Sumney was Chairman of VHSIC Steering Committee and served as Executive Secretary on the VHSIC Executive Committee.

Mr. Sumney has served on numerous Navy and ODD technical and management panels. He also was appointed as Executive Secretary to a Defense Science Task Force that has assessed the overall status and direction of the VHSIC Program.

On May 1, 1982, Mr. Sumney assumed the duties of Executive Director of the Semiconductor Research Corporation (SRC), a subsidiary of the Semiconductor Industry Association. In September, 1984, Mr. Sumney became President of the SRC. The principal purpose of the SRC is to structure an industry-sponsored, cooperative research program, responsible to the industry's needs, to be executed principally through research efforts in U.S. universities. The SRC will also be able to support research that is longer term in nature or more speculative than the industry would normally support. In this way, the SRC will make available to the U.S. industry a broader array of technology alternatives than each company would normally have at its disposal under current practices.

Mr. Sumney was a Technical Program Committee Member on the Device Research Conference in 1976 and 1978. He was Technical Program Chairman of GOMAC 78 and was General Chairman of GOMAC 80. He is a Senior Member of the IEEE and has been a member of the IEEE Field Awards Committee and currently serves on the IEEE Editorial board of *Spectrum*.

STATEMENT OF LARRY W. SUMNEY, PRESIDENT, SEMICONDUCTOR RESEARCH CORP., RESEARCH TRIANGLE PARK, NC

Mr. SUMNEY. Thank you, Mr. Chairman, distinguished members of the task force, ladies and gentlemen. In my testimony this morning I would like to briefly attempt to compare research project selection procedures in the context of the broader issue of management of research, so as to arrive at recommendations that may benefit the national research effort in significant ways.

In the international economic and technological competition in which this country is now engaged, this country no longer has the luxury of a dominating lead. Most of the positive elements of the future which we envision for ourselves—health, nutrition, productivity, defense, information management, communications, entertainment, and others—are and will continue to be strongly dependent on technology. We are all aware that in certain key areas of technology this country is losing its leadership. If this trend continues, the opportunities available to our citizens in the future will be greatly diminished. One can rightfully ask, what do we need to do to reverse the trend?

Technology is a product of research. This country continues to invest much more in research than any other country. There are two reasons why this is not reflected in greater technological success: First, I believe there is a lack of motivation within our system to adequately follow through with the application of research output; and second, there are inherent inefficiencies in the research process itself. This morning in my testimony I address only the last issue, that of inefficiency.

Inefficiency in research stems from a variety of factors. I would like to mention four:

First, institutionalization of the research establishment so that laboratories and programs often are continued even after their mission no longer merits continued investment, and when productivity has become marginal.

Second, lack of effective overall management often means that funding is directed to redundant efforts pursuing what may be the latest fad in a research area. Logic dictates that in highly important areas, multiple competitive efforts are desirable. However, in both industry and Government, research redundancy is sometimes carried to excess because of the perception that the largest rewards may accrue to the winner of such major competitions.

Third, because of difficulties in the effective dissemination of research results, research is often repeated because of inadequate knowledge of previous results. Associated with this is the difficulty, in this country, of one company, or one agency accepting research input from other organizations. This is often called the "not-invented-here" syndrome. Our society tends to reward the creation of new concepts rather than the adoption of older ones. Our perception is that a large percentage of existing research efforts are repetitive, trying to rediscover what is already known.

Diffuse leadership, my fourth point, results in a general lack of knowledge about the overall goals of the Nation, industry, a particular company, or the Government agency. In other words, we lack an overall strategy that includes relevant research efforts that can be available to those who are making decisions.

Before proceeding further, I think it's necessary to note that the United States' research establishment is still the envy of all competitors in the world. However, to compete, I think it must improve. There is also some difficulty in separating research from development. In my testimony I use research to describe efforts to increase the knowledge base no matter where it is applied. When a specific product is then identified, the research becomes development.

In this environment, specific research efforts may be implemented based on, number one, peer review, where experts in the same field judge the relative worth of the effort based on its potential for contribution to that field of knowledge. For this group, scientific advances are very important.

Second, user review. Here the potential users of the research results judge the effort based on its potential for contributing to the need. For this group, relevance becomes most important.

Third, management selection. Here research managers apply their judgment on the value of the research to their organization and to its goals. For this group, the overall research strategy of which the specific effort is one component becomes the most important.

And lastly, political selection. Here research is performed in order to provide benefits to an organization, a particular locality, or a research area. Factors external to the research effort are then most important in political selection.

Each of these procedures has its place and, properly applied, can provide efficiency to the research establishment. In fact, most research selections involve some degree of all of these procedures.

Now, in the organization for which I'm president, the Semiconductor Research Corp., our project selection involves, in descending order of importance: user review, management selection, peer review, and political selection. Because our mission is to enhance the competitive position of the semiconductor industry, an industry which has fallen upon hard times recently, relevance as defined by our member companies is most important. A high degree of management is applied to provide a balanced, goal-oriented program of research, and the opinion of peers is then used to assure quality. Our use of peer review is not a formal procedure, however. A technically competent staff is essential. The SRC has one and it interacts with the research community and takes note of peer-reviewed publications to provide this essential quality control element. The political content of our selection process is expressed by consideration of the desirability of investing seed money for new research resources, providing research efforts that are close to the users, and encouraging research at institutions that have traditionally responded to a particular area that we may have interest in.

In comparing our selection process to those of other research supporting organizations, it's necessary to separate fundamental research and applied research. First, peer-reviewed fundamental research is necessary to constantly replenish the scientific base of our society. The magnitude of the effort required for this category calls for a separate discussion.

The peer review process that is applied has its defects, but better alternative methods have not surfaced. However, once the research is directed towards goals and needs, the peer review process should be moderated to take these goals and needs into account. Most importantly, the selection process should include access to information on other related research and consideration of the overall strategic framework into which the project might fit. The research selection process will only be as good as the information on which it is based. Most funding decisions today are correct in the narrow perspective in which they are made. It is the broader perspective that is often lacking and which creates the inefficiency in the process.

Second, mission-oriented research decisions are made in a variety of ways. All too often they are made either at the bottom of the organization, without knowledge of overall strategy, or at the top, where the decisions are often remote from knowledge of either the technology or the implications of the decision. Just recently we heard of a decision in a Government funding organization that truncated an effort on a tool that is absolutely essential to the semiconductor industry. That decision was made at a high level, probably without knowledge of its implication and for unrelated reasons. We desperately need to obtain a closer relationship between the decisionmakers and those with knowledge of the broad technology issues.

Generally, the present system of many potential sources for research support, operating more or less independently both in industry and in the Government, assures that worthy research efforts are supported. If the process were more efficient—just, say, by 10 percent—then much more research could be supported. Individual researchers, who always believe they have a better idea are sometimes disappointed, but may not know that other equally competent scientists are already working on their idea and may well be ahead of them. If they were knowledgeable of this other research, or could identify the gaps in a broad strategy to which they could respond, then they might target their efforts more closely and obtain greater success.

With respect to the manpower burden associated with the review process, we at the SRC attempt to keep the real costs of reviews and selections at a level that is a small fraction of the projected cost of the research being planned. Inflexible selection procedures often ignore the size of the research project or the contributed costs of the reviewers or their organizations. In other words, it's an issue of perspective. Including overhead, a man-day of a competent reviewer costs somewhere around $500 or more. If 10 reviewers are convened in a meeting, the total costs can easily exceed $20,000. Appropriate levels of review are required in order to respond to the need for overall greater efficiency in research.

In summary, I am not arguing for radical changes but, instead, for the evolution of an information resource that would allow both researchers and research supporters to operate with better and more complete knowledge, enhanced leadership and management structure for research based on this better knowledge, and finally, for increased coordination and cooperation between the various funding agencies. Selection processes should involve a small number of competent reviewers that include managers with knowledge of the bigger picture and with access to knowledge on the existing activity level. The SRC provides a good model for this. Over 60 companies support our research effort in the United States and they are pooling their research support funds and helping to evolve an appropriate strategy in a cooperative effort that addresses most of the problems that I've discussed. Because the industry is intimately involved, the dissemination and application of research results is showing success. This country needs more of this type of cooperation, I believe. We are working hard to include the Government in the Semiconductor Research Corp. as a supporting partner. Industry and Government working together, in ever-increasing spheres of activity, I believe can solve many of the current problems and improve the efficiency of the research process.

Thank you.

[The prepared statement of Mr. Sumney follows:]

STATEMENT
BY
LARRY W. SUMNEY
PRESIDENT, SEMICONDUCTOR RESEARCH CORPORATION

ON
MANAGEMENT OF RESEARCH

The selection of research projects is inherently associated with the management of research. In this discussion, I will attempt to compare research project selection procedures in the context of this broader perspective so as to arrive at recommendations that may benefit the national research effort in significant ways.

In the international economic and technological competition in which this country is now engaged, the U.S. no longer has the luxury of a dominating lead. In order to maintain our position as a country that is able to provide strong leadership to the world as well as an excellent quality of life for its citizens, we will have to do a better job. Most of the positive elements of the future which we envision for ourselves--health, nutrition, productivity, defense, information management, communications, and entertainment--are strongly dependent on technology. We are all aware that in certain key areas of technology, this country is losing its leadership. If this trend continues, the opportunities available to Americans in the future will be greatly diminished. What can be done to reverse this trend?

Technology is a product of research. The U.S. continues to invest much more in research than does any other country. There are two reasons why this is not reflected in technological successes: a lack of motivation within our system to adequately follow through with application of research output, and inefficiencies in the research process itself. Here, I address only the last issue.

Inefficiencies in research stem from a variety of factors. These include:

1. Institutionalization of the research establishment so that laboratories and programs are continued even after their mission no longer merits the investment, and their productivity has become marginal.

2. Lack of effective overall management means that funding is directed to redundant efforts pursuing the latest fad in a research area. Logic dictates that in highly important areas, multiple competitive efforts might be desirable in order to assure a successful result. However, in both industry and government, research redundancy is carried to excess because of the perception that the largest rewards accrue to the winner of such competitions.

3. Because of difficulties in the effective dissemination of research results to where research is often repeated because of inadequate

knowledge of previous results. Associated with this is the difficulty in the U.S. of one company or agency accepting research inputs from other organizations. This is the so-called "not-invented-here" syndrone. Our society rewards the creation of new concepts, not the adaption of old concepts. Our perception is that a large percentage of existing research efforts are repetitive, rediscovering what is already known.

4. Diffuse leadership results in a general lack of knowledge about the overall goals of the nation, industry, company, or agency; in other words, we lack an overall strategy that includes all relevant research efforts.

Before proceeding, it is necessary to note that the U.S. research establishment is still the envy of all competitors. However, to compete, it must improve. There is also some difficulty in separating research from development. I use research to describe efforts to increase the knowledge base, no matter how applied. When a specific product is identified, then research becomes development.

In this environment, specific research efforts may be implemented based on:

1. Peer review - in which experts in the same field judge the relative worth of the effort based on its potential for contributions to that field of knowledge. For this group, scientific advances are most important.

2. User review - in which the potential users of the research results judge the effort based on its potential for contributing to their needs. For this group, relevance is most important.

3. Management selection - in which research managers apply their judgement on the value of the research to their organization and its goals. For this group, the overall research strategy of which the specific effort is one component is most important.

4. Political selection - in which research is performed in order to provide benefits to an organization, locality, or research area. Here, factors external to the research effort are most important.

Each of these selection procedures has its place, and properly applied can provide efficiency to the research establishment. In fact, most research selections involve some degree of all four procedures.

In the Semiconductor Research Corporation, our project selection involves in descending order of importance: user review, management selection, peer review, and political selection. Because our mission is to enhance the competitive position of the semiconductor industry through generic research, relevance as defined by our member companies is most important. A high degree of management is applied to provide a balanced, goal oriented program of research and the opinion of peers is used to assure quality. Our use of peer review is not a formal procedure. A technically competent staff interacts with the research community and takes note of peer-reviewed publications to provide this essential quality control element. The "political" content of our selection process is expressed by consideration of the desirability of investing seed money for new research resources, providing research efforts that are close to users, and encouraging research at institutions that have tradionally responded to the needs of the industry either through research results or manpower. Overall, we are pleased with the results being obtained.

In comparing our selection processes to those of other research supporting organizations, it is necessary to separate fundamental research and applied research. First, peer reviewed fundamental research is necessary to constantly replenish the scientific base of our society. The magnitude of effort required for this category calls for a separate discussion. The peer review process that is applied has its defects, but better alternative methods have not surfaced. However, once the research is directed toward goals and needs, the peer review process should be moderated to take these into account. Most importantly, the selection process should include access to information on all other related research (no matter where it is being performed), and consideration of the overall strategic framework into which the proposed project might fit. The research selection process will only be as good as the information on which it is based. Most funding decisions today are correct in the narrow perspective in which they are made. Broader perspective is often lacking.

Mission oriented research decisions are made in a variety of ways. All too often they are made either at the bottom of the organization without knowledge of strategy, or at the top, remote from knowledge of either the technology or the implications of the decision. Just recently, we heard of a decision in a Government funding organization that truncated an effort on a tool essential to the semiconductor industry. It was made at a high level, probably without knowledge of its implications and for unrelated reasons. We desperately need to obtain a closer relationship between the decision makers and those with knowledge of the broad technology issues.

Generally, the present system of many potential sources for research support, operating more or less independently both in industry and in the Government, assures that worthy research efforts are supported. If the process were more efficient, say 10 percent, then much more research could be supported. Individual researchers, who always believe they have a better idea, are sometimes disappointed, but may not know that other equally competent scientists are already working on their idea, and may be well ahead of them. If they were privy to the knowledge of this other research or could identify the gaps in a broad strategy to which they could respond, then they might target their efforts more closely and obtain greater success.

With respect to the manpower burden associated with the review process, the SRC attempts to keep the real costs of reviews and selections at a level that is a small fraction of the projected cost of the research being planned. Inflexible selection procedures often ignore the size of the research project or the "contributed"costs of the reviewers or their organizations. Including overhead, a man-day of a competent reviewer costs someone $500 or more. If ten reviewers are convened in a meeting, the total costs can easily exceed $20,000. Appropriate levels of review are required in order to respond to the need for greater overall efficiency in research.

In summary, I do not argue for radical changes, but instead for the evolution of an information resource that would allow both researchers and research supporters to operate with better knowledge: enhanced leadership and management structure for research based on this better knowledge; and increased coordination and cooperation between the various funding agencies. Selection processes should involve a small number of competent reviewers that include managers with knowledge of the bigger picture and with access to knowledge on the existing activity level in the research being considered. The SRC provides a model for this. Over 60 companies are pooling their research support funds and helping to evolve an appropriate strategy in a cooperative effort that addresses most of the problems discussed. Because the industry is intimately involved, the dissemination and application of research results is showing success. This country needs more of this type of cooperation; we are working hard to include the government in the SRC. Industry and Government working together, in ever increasing spheres of activity, can solve many of the current problems.

PANEL 1 DISCUSSION

Mr. **Packard**. Thank you, Mr. Sumney, for your excellent testimony.

Let me begin with questions of you, Mr. Sumney, and then we'll proceed to others.

You stated that one of the inefficiencies in our research process lies in the continuation of laboratory and programs after their mission no longer merits the investigation of their productivity, or their productivity has become marginal. This argument tends to be levied against the duration of the awards that are being given, and in other testimony we've heard this week there's been a desire and a request for longer and more stable funding periods.

Can we infer from your statement that we should maintain our highly competitive peer review process for selection of short-term projects? How do you equate the concern for longer term and more stable funding of projects recognizing that your statement would tend to indicate otherwise?

Mr. **Sumney**. Well, I think it's all a matter of perspective. I believe that peer review is absolutely essential for many of the very fundamental research areas. Research projects, once they're underway, gain a certain momentum, and I think that my statement was meant to imply that you need to continue to look at the output of the research efforts and the relevance of what is going on.

This does not necessarily imply that you're favoring short term. It could be a 10-year program. But once that program becomes less relevant than it was when it started, once the output becomes of less impact than when it was started, then I think it takes a hard management decision to say that that particular research program needs to wind down, that particular laboratory perhaps needs to reorient its mission, or what have you.

I think the problem we're discussing is not limited to the Government. Large industrial corporations have the same problems in their research programs. We see a number of examples where it continues because scientists are very closely affiliated with it, they feel a part of it, and it's difficult oftentimes to start it. Yet it is inefficient to continue it.

Mr. **Packard**. You referred to the inflexible selection procedure that exists now, and also you have certainly spoken considerably on the need for increased efficiency in the review process.

What specific recommendations would you make to improve the efficiency of the process and to perhaps bring about additional flexibility in the system?

Mr. **Sumney**. I guess my recommendations would call for a balanced selection process, where appropriate. We at the SRC, as I indicated in my testimony, emphasize user need as the primary criterion for selection of a research project. Peer review is included as well, but it's not the primary aspect.

I think that basic research can be pursued in many different areas, as we all know. There is nothing to say, though, that that basic research that you want to do should not relate to goals and objectives. We strongly feel that efficiency comes about by trying to identify overall research strategy that includes goals and objectives for where you want to go. Then you have to balance your research procedure. You may want to involve peer review, but you have to have the goals and objectives there as well. It is the intermingling or the coordination of these various factors which I think can improve the efficiency.

Mr. PACKARD. Thank you.

Dr. McCarty, your testimony and the way you operate at the Whitney Foundation I think indicates that you support the concept of picking the right individuals—strong, well experienced, competent researchers—in preference to selecting specific projects. Many foundations and voluntary organizations select basic research projects solely on a peer review system—that's the procedure that most of them follow. Whitney, again, generally moves toward the promise of the proposed fellow.

Do you have any information to compare the success rate of those chosen under your system versus those that might be chosen by a project peer review system?

Dr. MCCARTY. We don't have such information at present, I think in part because the fellowship selection tends to be based on the individual rather than project. There are a limited number of situations where the area of the research becomes the dominant factor, but I don't think there's any group of comparable size or duration that would allow us now to make such a comparison.

This is more like the sort of thing that the idea behind research projects in general is to get the young individual with the highest promise and let them have their head in their research. This is certainly a very common philosophy.

Mr. PACKARD. Should the Government, in your judgment, change its procedure, where we now tend to use the peer review system and project-oriented system far more than we do any other system?

Dr. MCCARTY. Well, I would say, in the fellowship selection part of the Government program, it certainly is a peer review operation, but it does tend to focus on the individual for a postdoctoral fellowship more than on the project. I think probably, in essence, they would evaluate the fellows pretty much at the same level that we do with the individual promise coming first and the project and the area of the research coming as a second consideration. So that it's a little bit different from the research grant program, where I agree the primary aim of the peer review system is to look at the project, always considering the competence of the principal investigator, of course. But I believe that's more applicable to the research grant than it is to the Postdoctoral Fellowship Program.

Mr. PACKARD. If the Government made a change or a shift more to looking at the researcher in preference to the project, how would that, in your judgment, change the quality of research taking place under the auspices and funding of the Federal Government?

Dr. MCCARTY. Well, in my view, it probably would change it relatively little, simply because the current process does look at both, and a person without proven ability in research is not likely to get chosen, regardless of the project which he selects. Therefore, it still involves both components in a major sense.

Mr. PACKARD. Does it require a different reviewing system when you are looking at an individual versus a project?

Dr. MCCARTY. In a sense, I believe it does. It requires a different focus because we, in effect, do not even expect or require the individual to carry out the project as he describes it. He is describing the project as a fellowship program that he thinks is appropriate at the time that he applies. But because of developments or because of changes in the situation, he may, by the time he gets to his fellowship a year later after his application, take a quite different tack. Therefore, I think you have used the project in the fellowship application as a basis for assessing the ability of the applicant to set up a program and outline what he proposes to do, rather than having that as the major consideration for selection.

Mr. **Packard**. Does it give well-experienced, well-entrenched scientists or researchers, with a good reputation and a good track record, a much better advantage than a young, enterprising, bright, and new researcher? Is it harder for the new researcher to break into the system under that procedure?

Dr. **McCarty**. I suspect it probably is still somewhat harder. One of the things that one hopes to achieve with the postdoctoral fellowship program of the kind that we have is to give the scientist time—after he has his degree—to establish himself, to get a track record, to improve his chances of competing for research grants on the basis of the postdoctoral training. I think this is one of the big areas of importance for the postdoctoral training period. So they are competitive.

Mr. **Packard**. At Whitney do you have any special procedures that you employ in an effort to capture the young one or to give special consideration to the young enterprising researcher?

Dr. **McCarty**. Our applicants are required to be recent doctoral degree people. In other words, if they've already had more than one to two postdoctoral years, we do not entertain their fellowships. So this is primarily an early postdoctoral training program.

Mr. **Packard**. Thank you very much.

Dr. Brinkman, since World War II the United States has supported basic research for science sake. As I think your statement accurately states, we have constantly been a leader in terms of the amount of dollars we put into basic research. I think also one of your concerns is that we do not transfer that basic research to a product and are able to compete internationally, at least in comparison to the amount of money we put into basic research, into applied research. We do not compete well in that area.

The question, I guess, is, do you think we can continue to afford to plow significant amounts of our research dollars into basic research when we're not really capturing the potential in terms of marketable products?

Dr. **Brinkman**. Well, in my opinion, I think there's no question that we can afford that. In fact, I don't think that's the issue. The fact is we're putting a very large amount of money in R&D in this country, $100 billion or so a year, and the basic research part of that is a very small fraction—maybe 7 percent all total for both biological and physical sciences. That is not a big number.

In some sense, that amount of money is not the issue as far as getting the transfer to relevant technology. Somehow the country does not seem to have the right mechanisms and structures that feed into industry in a constructive way. I think there has been a singular lack of marriage between the U.S. Government and industry in this country to see that that moves forward.

That is a different issue from funding the basic research. It seems to me this country surely wants to keep the thing that it's doing well, and that is, its basic research component, in trying to address the real problem of what is wrong that it doesn't manage to stay technologically ahead. It seems to me those are separate issues.

You know, attacking basic research on this issue seems to me is sort of attacking the good part of the thing we're doing and dragging it down with the things we're not doing well. And so the question is not whether we should continue going into basic research; I think we should continue to try to see to it that, in fact, we do as well as we can in that area, and then try to solve the problems that exist elsewhere.

Mr. PACKARD. Do you think the Federal Government is not doing enough in making that transition? Should that continue to be left primarily with the private sector?

Dr. BRINKMAN. I think the Federal Government could do more to help that transition, and I think there are ways that the Federal Government can help, in the sense that I think our national laboratories, which are more directed towards missions so that you get your complete spectrum of research, would be a more appropriate thing.

For example, we work in the semiconductor business. We have a radiation-hardened semiconductor facility at Sandia, and by having laboratories involved in that particular technology, it is absolutely necessary for the weapons program and for a lot of other things that we do basic research that contributes to that field. I think we have a very high spectrum of activities that come from the most basic questions and fundamentals of solid state physics over into how you harden a semiconductor device. That kind of thing the Government can do more of and try to examine these kinds of issues to see to it that there is this spectrum of things going on and properly coupled and correlated.

I do not see that the Government should try to say, "Gee, we're doing this part bad so we ought to kill the part that we're doing good." That just doesn't make any sense at all.

Mr. PACKARD. Thank you.

Mr. Fuqua.

Mr. FUQUA. Mr. Sumney, you were talking about the lack of transfer of information, the duplication of a lot of research from one discipline to the other parts of that discipline.

Can you elaborate on the difficulties of that? It would seem to me, particularly in the last 10 years, when we've had a compilation of a lot of research projects that have been published that there would be—I don't say there's a central storehouse; maybe in some cases there is—but at least scientists could find out what was going on in their own field through seminars, meetings, and so forth.

How widespread is this and how serious is it?

Mr. SUMNEY. I think it's reasonably widespread. We find the problem in many of the areas where we're trying to fund research. I think there are mechanisms—and certainly the technology exists—to make the process more efficient. It's a function often of the individual researcher. Sometimes it's a function of the institution where he finds himself.

For instance, at the SRC, we have put together an information base with a host computer that makes all the results in our research program available to all the researchers that we are funding, so that they can keep up-to-date with the results that are coming out of our program, without having to go to hard copy. It's all right there in front of them on their terminal. All the abstracts are available in real time.

Mr. **Fuqua**. This is a network that you can hook into to find out what's going on?

Mr. **Sumney**. Yes.

Mr. **Fuqua**. Say at MCC?

Mr. **Sumney**. Yes; well, MCC is a little bit different because MCC has a compartmentalized research program. Our system accesses the ARPANET. So, through that, we have access to a broad range of research output. For those programs that are compartmentalized, like MCC, we can't get into that necessarily. But yes, we're aware that our system has interface with others and we're doing that.

Mr. **Fuqua**. Would you say that applies, say, for chemistry, biology, mathematics, physics?

Mr. **Sumney**. Certainly the ones that are moving rapidly it applies to, yes, certainly in the area of biology and chemistry, physics. More and more, all of these areas are becoming very interdisciplinary. Semiconductor research involves manufacturing of devices, and that involves operations research and manufacturing management. There's design, there's physics, mathematics, computer science, computer engineering, chemistry, physical chemistry, and a broad range of disciplines that are involved. So more and more things, as they advance, are becoming more and more interdisciplinary to make the improvements that are required.

Mr. **Fuqua**. Dr. Brinkman, the mission agencies like NIH, DOE, USDA and NASA—and they have varying objectives in mind when they support research—are they advancing science or advancing the objectives of their own agency when they support this?

Dr. **Brinkman**. Well, I think it depends on the agency you're talking about. Some of them are better and some of them are worse. I think that if it's properly managed you end up advancing science as well as your mission. We think we do that in the sense that the way we handle it is, while we are choosing projects, choosing areas of research that we think are going to be relevant to our mission, we try to choose absolutely the best research people we can find in those areas and have people who are trying to contribute and compete in that scientific community like any other scientist.

Mr. **Fuqua**. I guess I'm asking, too, do we need to broaden the peer review process in those areas to include maybe engineers, medical doctors, or other disciplines that might be involved in that, rather than just strictly in that one narrow discipline?

Dr. **Brinkman**. That's an interesting challenge. In the peer review process, when it comes to reviewing one particular technical paper, you have to get technical expertise. However, to try to shift the general emphasis of different areas and different subfields, you should have a broad spectrum of inputs to try to understand exactly what areas are important.

If you're running NASA—presumably I would have thought that NASA would want to try to balance its program in such a way that it looks for what kinds of technologies it sees in the future of the space program and raise or lower the relative support of the various research efforts that it manages in such a way that it's consistent with that.

We try to do that in a continuous way. For example, a number of years ago, when we got into the energy programs, we started a geosciences research program and deemphasized some of our other programs. We have increased substantially over the years our computer science effort, trying to become stronger in that, because we thought it was relevant to our long-term nuclear weapons command and control issues, things like that. So you can't judge a particular proposal with generalists, but generalists have to look at the larger picture. So you have to combine these things in a rather complex way. Your job, of course, is to look at even a larger picture yet, to the whole total effort of the Nation. That's the kind of way it has to work, it seems to me. But peer review for a specific project obviously needs expertise.

Mr. Fuqua. Dr. McCarty, in answer to a question by Mr. Packard, you said that in the Whitney Foundation you were placing more emphasis on the individual rather than the science, and I realize that is well within the objectives of that foundation.

But for Government-supported basic research—and you have been involved and have a very distinguished career—how do we involve or encourage the young investigator or the new investigator, depending on whether you want to be called young or old or not, into feeling that they can get a grant and become involved in the process?

I'm not really speaking from your background as a foundation.

Dr. McCarty. Yes; well, my perception of this, during the period I was most heavily involved, was that the way that the young investigator got in a position to compete in our current peer review system was to have this postdoctoral period in which he was supported by some other source of funds in which he established himself. And many of them do. Of course, it varies with luck and the chance of what goes on. But this is, I think, effective in a fairly substantial proportion of the cases.

In the period that we support—for 3 years—for example, in the postdoctoral period, and many other agencies do as well, during that period quite frequently the man who has had very good predoctoral training will establish himself and make a mark that really makes him competitive at the peer review of a research grant. So I think this is the way one would hope it would work.

Now, obviously I think the concern is that when the funds become tight, then even the better of the young people coming along have trouble competing with the fully-established investigator, and I suspect that means that one has to, as people have certainly tried to do, to modify the system so that there is some attention paid to the upcoming investigator who is so important to the future of the development of facts.

Mr. Fuqua. Thank you.

Thank you, Mr. Chairman.

Mr. Packard. Thank you.

Mr. Brown.

Mr. **Brown**. Just to follow up, Dr. McCarty, I perceive from your statement that the Foundation has been doing an excellent job in bringing promising young researchers into the biomedical field, and we have made great progress in that field. But as you probably know, there have been certain driving forces in the biomedical field that don't exist in, say, the plant biotechnical field. As a consequence, we have lagged in the applications of fundamental molecular biology in the plant field. And maybe that's appropriate. Maybe the rewards would not justify any more than we've done. I'm inclined to feel that they would.

But my question is, on the assumption they would, how do we go about providing that emphasis into plant biotechnology based upon the same fundamental science that you have been so active in developing, that we have in its applications to human health?

Dr. **McCarty**. Well, I think once you have the possibilities, some kind of a program which allows those that become interested in this area to get support, to carry it out, it probably will gradually evolve. I can say, for example, even in our program, which as I said is designed for basic biomedical research, we have now had a small group, to be sure, of individuals who in the course of their training have decided it is of great importance to apply molecular biology to plant systems. So we now have several of our former fellows who are in this area, having gotten their training in the biomedical end of it.

Mr. **Brown**. You didn't prohibit them from going into it.

Dr. **McCarty**. Exactly. We did not. We actually will even support that as being basic biomedical science, because when one gets to worrying about, say, nitrogen fixation at a molecular genetic level, it makes sense to consider that a perfectly reasonable basic approach.

Mr. **Brown**. I wish we had more time to discuss with Dr. Brinkman and Mr. Sumney the statements that they have made.

Dr. Brinkman, you left an interesting challenge in the last paragraph of your statement, when you suggest the possibility of using the Sandia model as the national model. That is precisely what we've been trying to do in this committee for a number of years, to develop a national model for providing that integrated coverage of the whole chain from basic research to its ultimate social utility. We haven't succeeded yet. I want to let you see if you can do it.

Dr. **Brinkman**. Thank you. I would love to try.

I think it is a very difficult thing, and a national model is never going to be as easy to run as a microcosmos such as Sandia. There is much more randomness in the system. But what I would suggest is that if people think about the totality of the effort in that way, thinking about what you're trying to do—namely, what's important to the country and what kinds of things you're trying to balance—you clearly want to have a whole set of ingredients in that measure. Namely, the intellectual stimulation of the scientific community is one thing, but the fact is we also have all our technological-economic goals in this country and we ought to be thinking of our research in that context.

Mr. **Brown**. We have no institutional focus for doing that kind of thing.

Dr. **Brinkman**. I understand.

Mr. **Brown**. You do at the laboratory.

Dr. **Brinkman**. We do because we're a smallish place. There is no institutional focus, and I would suggest that is something you need to think about, a way in which that could be done.

I would assume that that's really the role of the OSTP, the Presidential Science Advisor and places like that. They are supposed to do that, right?

Mr. **Brown**. Well, let me give you an interesting example. We wrote into the OSTP Act—this is in line with Mr. Sumney's remarks about the efficiency of research, requiring a scientific and technological data base, knowledge base—we wrote that specific requirement in rather precise language into the OSTP Act and nothing has been done about it.

Now, that's the problem.

Dr. **Brinkman**. I'm afraid I can't answer your question on how to solve that problem. But you understand what the problem is. I think it's very important to get that kind of perspective somewhere.

Mr. **Brown**. You, Mr. Sumney, have indicated the desirability of having Government participation in the Semiconductor Research Corp. We have tried for years, through things like the Stevenson-Wydler Act and others, to encourage that, and we're moving backward, not forward, at the present time.

Mr. **Sumney**. Our approach to help stimulate the participation is that we work very closely with NSF and we are now going to various national laboratories to look at what their capabilities are. We just recently visited Sandia. We are planning on visiting Brookhaven and Livermore in the near future, to look at their capabilities and inform them of what our goals and objectives are in our research program.

What we have tried to do is we have tried to structure a scenario for where the industry wants to be in 1994 and try to enumerate specific goals. Our researchers are working toward those goals and we're trying to communicate to the national laboratories those goals as well, to see if there is common interest on the national laboratories contributing to it.

Mr. **Brown**. Well, I don't want to belabor this at this point, but we're tentatively thinking of 2 or 3 days of hearings on this precise subject later on in the summer. Maybe you would like to come back.

Dr. **Brinkman**. I would love to.

Mr. **Brown**. And testify at that point on this specific kind of a situation.

Mr. **Packard**. Thank you, Mr. Brown.

We want to thank the panel for being with us, Dr. Brinkman, Dr. McCarty, and Mr. Sumney. We appreciate your testimony and the answers to the questions.

Before we move to the final panel, I think we'll take a 10-minute break, or as long as it's necessary for us to go vote. Then we'll return.

[Whereupon, the task force was in short recess.]

PANEL 2

Mr. **Packard**. We will call this hearing back to order, ladies and gentlemen. We are pleased now to have as our final panelists members of the private sector that I think will give us, hopefully, some input that will reflect the recipients of grants and those that provide some of the research activities, a perspective for us as to the procedures that we're using.

We are very pleased to have with us Dr. John Armstrong, who is vice president, logic and memory, IBM Research Lab from New York, and Dr. Richard K. Quisenberry, who is director of the research division of the Du Pont Experimental Station in Wilmington, DE. We are pleased to have both of you here.

We would like now to hear your testimony, Dr. Armstrong, first.

[A biographical sketch of Dr. Armstrong follows:]

Dr. John A. Armstrong

Dr. John A. Armstrong was born in Schenectady, New York, in 1934. He received an A.B. in Physics from Harvard College in 1956, and his Ph.D. in 1961 from Harvard for research in nuclear magnetic resonance at high pressures. He then, as a Research Fellow, switched into lasers and nonlinear optics working with Professor N. Bloembergen. In 1963 he joined IBM Research as a Staff Member. He spent the year 1967–68 at the IBM Research Laboratory in Zurich, and returned to Yorktown as Manager of Quantum Optics. Between 1976 and 1980, as Director of Physical Sciences, he was responsible for a major part of the physics, chemistry, and materials science at IBM Research. In 1980 he joined the IBM Corporate Technical Committee headed by IBM Chief Scientist, L.M. Branscomb. In 1981 he was made manager of Materials and Technology Development at the IBM East Fishkill Development Lab, working on advanced bipolar VLSI technology and associated packaging. In 1983 he returned to the Research Division as Vice President, Logic and Memory.

Dr. Armstrong is author or co-author of more than 45 papers on the subjects of nuclear resonance, nonlinear optics, the statistical properties of laser light, picosecond pulse measurements, and the multiphoton laser spectroscopy of atoms. He is a Fellow of the Optical Society of America, a Fellow of the APS, a member of the DSSP, and a Fellow of AAAS. He was North American program chairman for the IXth International Quantum Electronics Conference. He was a member of the Assembly of Mathematical and Physical Sciences of the National Research Council, of the Subcommittee to Review NSF-supported Nuclear Physics Laboratories, of the Committee on the Employment of Foreign Scientists in the U.S. (of the National Research Council Commission on Human Resources), was Chairman of the Advisory Committee for Physics of the National Science Foundation, and is a member of the Engineering Research Board of the National Research Council, a member of the Commission on Engineering and Technical Systems of the NRC, and a Trustee of Associated Universities, Inc.

STATEMENT OF DR. JOHN A. ARMSTRONG, VICE PRESIDENT, LOGIC AND MEMORY, AND DIRECTOR, SEMICONDUCTOR SCIENCES AND TECHNOLOGY, IBM THOMAS J. WATSON RESEARCH CENTER, YORKTOWN HEIGHTS, NY

Dr. **Armstrong**. Thank you very much, Mr. Packard, and members of the committee. It's a pleasure to be here and I appreciate the invitation.

Let me just give a word of background. It seems to me that the whole topic of choosing research projects has two major ingredients, and one of them is addressed in my prepared statement—and because I digress the hard one, there are two.

The hard part in an industrial research lab is deciding in which areas one should, in fact, do fundamental research, what is it that really is appropriate. I will talk in some detail about that.

Having done that—and that's a matter in which we have devoted a lot of thought and attention over the last 15 years in IBM research—it is fairly easy, although always requires care, to choose individual projects, using very many of the same procedures that we've heard discussed here this morning and using the model, in many cases, of the system by which university academic scientists choose their programs.

If you have hired really first-rate people, and if your management has been chosen from the intersection between the set of your very best scientists and a set of them who have some management talent, and if you have figured out what are the sensible areas in which to do both basic and applied research, it will go well.

Now, let me just say some of the three principal areas in which we have found that choosing applied research makes sense.

First of all, the materials and the processes that are used in the electronics industry, out of which chips are made, for example, are at the level of detail and scientific understanding required for today's technology almost as mysterious as if they had just come back from the Moon. There is a materials and process science gap between the state of scientific knowledge, let's say, with respect to silicon or silicon dioxide or silicides or magnetic films. There is a very substantial gap between the state of scientific knowledge and the requirements of modern technology. Moreover, the technology is moving at least as fast as science in most of these areas, so that gap is not getting any smaller.

So it is not hard to find a set of areas—and those are the first that I list here—it is not hard to find a set of areas if you're in the computer business, in the microelectronics business. It is not hard to find scientific topics which are guaranteed to be of importance to your ability to compete and which at the same time involve the most basic solid state physics and chemistry of the kind which would be absolutely at home in any major university. And so we focus a lot of attention in that area. This is what I would call sort of new science on old materials.

But there is new science there, and that's one of the messages that I often have to get across to my university colleagues who sometimes feel perhaps we know everything there is to know about silicon dioxide whereas, in fact, we know almost nothing of what we need to know about silicon dioxide.

Now, a second major area is what we call area science at IBM research. With a lot of thought as a result of some of us having spent some time in the development and manufacturing environments, we have been able to identify a half dozen to a dozen areas of what we call generic areas of science, chemistry, materials science—physics, chemistry, and materials science—where improved basic understanding is very likely to lead to important consequences for our business. A number of such areas would be the understanding of very thin films of materials, the whole topic of buried interfaces between materials.

You know, a computer chip is a big club sandwich or layer cake of materials, but the layers are so thin that the properties of most of those layers are not well understood from a scientific point of view. So, thin films interfaces, very low temperature processing, et cetera, are areas where there is real science to do and where there's a good chance that there will be long-term relevance to our business.

Now, in this sort of topic of area science is another one that, of course, is important to us, and we're always on the lookout for new phenomena, usually at very low power and in very small scale, which might be the basis of revolutionary new electronic devices. However, that's a very, very hard business. What's required are truly revolutionary new devices if a new device is to have any chance of competing against the juggernaut of established semiconductor technology. But new phenomena in quantum transport and ultrasmall structures, ballistic transport, are very important topics.

A final area—and then I'll leave this sort of survey of areas—another area in which we know that it is sensible to do both basic and applied research is in the area of advanced diagnostic and analytical techniques. I'll give you a primary example, which would be the techniques of x-ray photoemission spectroscopy of surfaces which have come out of the concern over the past decade with surface science. Brookhaven National Laboratory played a role in that, as most of the other large light sources have, even though the science that may have been motivating some of these new techniques has not yet proven to be of major importance for technology.

The tools that have been developed have been incredibly important, and so we are happy and thrilled, as a matter of fact, to be able from time to time to support the development of new scientific tools, even though at the time there's no really persuasive relevance to be made to our business. That will take care of itself, and I'll give you two examples.

Years ago, of course, we were heavily involved in the invention of the semiconductor laser, the tunable organic dye laser, and more recently the scanning tunneling microscope, which runs along the surface and looks at the surface atom by atom. These are examples of tools which will be very important to science but will also be very important to the sophisticated sort of diagnostics and research that has to be done to unravel this layer cake that is the basic structure that we build in the computer business.

So we have those three areas. There is this new fundamental scientific knowledge in areas of known importance to us, a lot of room for improvement, a lot of room for increased coupling with universities, a generation of new fundamental knowledge in generic areas of long-term probable relevance, and the generation of new and highly sophisticated tools. When we get our hands on a really bright young person, or, for that matter, a really bright old person, who has interest or ideas in these general areas—and they are quite broad and interdisciplinary—it is not particularly difficult to decide to commit the resources that are required to initiate and continue projects.

With that overview, I will pause for the moment.

[The prepared statement of Dr. Armstrong follows:]

Summary of Remarks prepared for The House Science and Technology Committee by J.A.Armstrong, IBM Research Div. Vice President, Logic and Memory: April 10, 1986

I have been asked to address the topic of "Research Project Selection".

First, let me state that I will be talking about both fundamental and applied research, but in neither case about work aimed at specific products . Although I will confine my remarks to the areas of solid state physics, chemistry, and materials science, the general ideas by which we choose projects are applicable in the other areas of research carried out in IBM Research, for example in computer science or in mathematics.

We select research topics in three main ways:

(1) Where we know in advance that new scientific knowledge will help our present and future technologies and products: e.g., what are the microscopic mechanisms which cause defects in the SiO_2 insulator used in memory chips? The answer to the question will involve absolutely basic solid state physics, will require very sophisticated equipment, and there is no risk that the answers will be irrelevant. There are many other examples here. The knowledge is guaranteed to be useful, but the research is of the most basic sort, perfectly suitable for pursuit in any university condensed matter physics and chemistry program.

(2) A closely related area is what we call "area science"......that is, after much reflection and discussion, we have identified a dozen or so areas of physics, chemistry, and materials science where improved basic understanding is likely to be of long term to technology. An example would be the understanding of thin films and interfaces in general, and of such things as low temperature processing, properties of silicides, reactive ion etching mechanisms, etc. etc.

Like the first category just mentioned, these topics in "area science" involve basic understanding, and will result in scientific publications and papers at conferences.

(3) Another category of our projects is that of advanced analytical and diagnostic techniques. X-ray photoemission spectroscopy of surfaces, or scanning tunneling microscopy, or sub-picosecond electrical pulses.

There is a very important point here; namely, that one of the most productive and rapid ways in which basic research can contribute to technology and productivity is through the invention, development, and sophisticated application of new and advanced tools. A remarkable recent example is the so-called Scanning Tunneling Microscope, invented at the IBM Zurich Research Lab, now being pursued, elaborated, and applied all over the world. This tunneling microscope is capable of revealing the locations of individual surface atoms on many different kinds of surfaces. There is intense fundamental interest in this tool; it will help with the solution of many long standing basic problems in surface science, but it is also immediately useful in diagnosing processing problems which arise in certain steps in VLSI processing.

Other examples of this commitment to the development of new tools for science and technology include invention of the semiconductor laser and the tunable dye laser, invention of means for measuring picosecond laser pulsewidths, and the design and construction of a toroidal grating monochromator for x-ray photoemission studies of surfaces.

In summary, at IBM Research we choose our basic research projects primarily in Three Categories: (1) Generation of new, fundamental scientific knowledge in areas of known importance; (2) Generation of new fundamenatal knowledge in certain generic areas of long term, probable relevance, and (3) Generation of new and highly sophisticated tools for research.

Mr. **Packard**. Thank you very much, Dr. Armstrong.

We will proceed now with Dr. Quisenberry.

[A biographical sketch of Dr. Quisenberry follows:]

Dr. Richard K. Quisenberry

Richard K. Quisenberry, director of the Research Division, Central Research & Development Department, joined the Du Pont Company in 1960 as a research chemist at the Kinston, NC, Textile Fibers plant.

After several research assignments at Kinston, he moved to Seaford, DE, in 1966 as senior research supervisor. He became research manager at Waynesboro, VA, in 1968 and was named technical superintendent at Martinsville, VA, in 1972. In 1973, he became manufacturing superintendent at Martinsville. In 1975, Mr. Quisenberry transferred to Corporate Plans Department in Wilmington as a principal consultant.

He returned to Textile Fibers Department in 1977 as marketing product manage for "Typar" spunbonded olefin and later that year became business planning manager for spunbonded products. In 1978, he was named product manager for "Kevlar" aramid fiber and the following year moved to the Central Research & Development Department as director, feedstocks research and development division.

In October 1980, Mr. Quisenberry was appointed director, pioneering and textile research for Textile Fibers. In January 1981, he was appointed director of the Research and Development Division, Finishes & Fabricated Products Department. He assumed his present position February 15, 1984.

Born July 27, 1934, in Springfield, IL, Mr. Quisenberry graduated from Millikin University in Decatur, IL, in 1956 and received a Doctor of Philosophy degree from the University of Utah in 1960.

Mr. Quisenberry is a member of Delta Sigma Phi social fraternity, Sigma Xi honorary fraternity and the American Chemical Society.

He married Miss Karen Reime of East St. Louis, IL, in 1957. They have four children, Lisa Diane, David Keith, Amy Kathryn, and Stephen Thomas. The family lives at 404 Way Road, Greenville, DE.

STATEMENT OF DR. RICHARD K. QUISENBERRY, DIRECTOR OF THE RESEARCH DIVISION, CENTRAL RESEARCH AND DEVELOPMENT, E.I. DU PONT DE NEMOURS & CO., INC., WILMINGTON, DE

Dr. **Quisenberry**. Thank you, Mr. Packard.

I have a written statement before you. I don't intend to read all of that to you. Let me just select from it as we go through and maybe highlight some of the key points and leave time for your questions.

Clearly, for those of us who manage basic research, regardless of where and what kind of a laboratory it is, the most important issue that I think any of us face is deciding what to do, and the corollary of that is what not to do.

While project selection is the most critical issue for those who manage science, in my view it must be considered, I think, in context with several other closely related activities. And these are, in a kind of chronological order: budgeting; statements of long-term organizational objectives which can be translated and understood by the technical community; formation of a quality staff; selection of specific programs or projects; systematic review of those projects; and, finally, deciding to quit or continue. In just the next few moments I'll talk on each of these briefly.

Before I begin, though, it might be appropriate to take just a moment and tell you something about Du Pont's research and development effort and something of its size.

We have a budget of about a billion dollars to support the research of 6,000 professional scientists during the current year. Our budget is composed of five parts. The parts that I think are the most important to this committee today are those that we call basic research and discovery research, and these two together make up about 15 percent of our total budget. It is those two parts that I have major responsibility for. In Du Pont, we feel pretty good about the role that science and technology has played in our company over the last 50 years, and in every sense we regard research and the basic component of it and its timely exploitation in the marketplace as absolutely critical to our future.

The other 85 percent, the lion's share of our research budget, is applied and directed research. The decision as to how that is spent is fairly easy, although quite often very painful in setting the priorities, but it is highly quantifiable and it is directly related to business objectives. After a lot of iterations, that budget is pretty well settled. The situation is not so clear cut with basic research. The management challenge for us here, which is quite analogous and every bit as difficult as the question that faces the administration of the National Science Foundation or the NIH, or the dean of a large graduate school, is deciding how to allocate these limited resources.

We don't have any special formulas, we don't use arcane analyses, and certainly we are not infallible. We have made a lot of mistakes in deciding what to do. But the fact that we're still in business suggests that we may be correct in our decisions more often than not.

Our process begins with the budget. Individual business units submit their requirements and, as I just indicated, through a process of iteration, we develop a research and development budget.

In basic and discovery research, however, the situation is quite different. The budget is not developed by the management of the business units but by the very top corporate management. The size of this budget—and, importantly, its shape and emphasis—conforms to the vision of the future of the Du Pont Co. as developed by the management team. As my colleague just indicated, rather than worry about specific research projects or programs, the research leadership and corporate management have a continuing dialog and agree on broad areas of scientific expertise we wish to establish and which we believe will enhance our success in pursuing the forward commercial objectives of the Du Pont Co. If we're correct in identifying these broad technology themes, the payoff to the company—and I think to the scientific community broadly—can be significant.

Let me give you an example of such a theme. Over the last 20 years we have worked hard to maintain an expertise—or, to use a term in our organization, a center of excellence—in the field of organometallic chemistry. This is a specialized branch of science which has become the scientific basis for catalysts, photoimaging and magnetic recording. This activity is directed by a scientist manager who has achieved such prominence in that field that he's the editor of that discipline's scientific journal and last year was elected to the National Academy of Sciences in recognition of his contributions to this area. He is currently on sabbatical leave at Oxford University in England.

I think few would argue the importance to Du Pont of being one of the leaders in this science in order to pioneer new catalyst-based chemical processes, x-ray based diagnostic systems, and magnetic recording for audio and video computers. We are now discovering that our major investment in this science has positioned us well to participate in the emerging business involved in the transmission and recording of information using optical technology as well.

In the handout I gave the committee earlier I listed some other areas of technical excellence or broad research themes that we're pursuing as examples.

In listening to the testimony earlier, Mr. Brown, I will tell you that one of these areas of technical excellence we have carved out for ourselves is, in fact, the molecular biology of plants.

Once these broad themes have been understood and agreed upon, two commitments are required. The first is based on the principle that a successful basic research program needs a horizon that is at least 10 years out and a commitment by all to that horizon. Second, success can only come through quality staff. Judging the quality of staff is a difficult issue. Our experience is that some sort of peer review and professional standings are the best ways to approach these questions. The peer review of which I speak is not the rigorous formal process that this committee and task force has heard about, but a more qualitative peer review that I'll be happy to discuss if you wish.

In most of our areas of basic scientific expertise we believe we have attracted a staff of principal scientists that compare favorably with the faculty of many of our leading universities. We are able to recruit and importantly retain quality scientists because, frankly, we think we have an attractive environment in which to do good science. We stress staff quality because in basic programs the specific research projects are generated by our principal scientists or, increasingly, multidisciplinary teams of scientists. We believe we commit generous support resources to basic research programs and, most importantly, keep administrative burdens and paperwork to a minimum.

Over half of our staff come from university appointments or faculty positions. They comment on how easy it is to propose and sell a research project in our labs and not have to go through the torturous grant preparation process that has become the norm in many academic laboratories.

We are committed to the concept of research projects generated by the scientific staff rather than formulated at the top and passed down by management. Our reason for this view is simple. Most important scientific discoveries and important new principles are made following the lines of investigation peripheral to the articulated objective of the project. Simply put, innovation cannot be forecast.

Our experience convinces us that the combination of a quality staff team pursuing programs to which it is both intellectually and emotionally committed will result in fundamentally new useful knowledge and those occasional breakthroughs that are so exciting and makes the often frustrating world of science so thrilling.

Another important element in ensuring success in basic research is the interaction between our scientists and their colleagues. We encourage strong collaborative interactions with leading scientists in universities and Government labs and in some cases in other industrial laboratories. We have a program to bring to our organization for a stay of a year or two leading academic scientists on sabbatical leave or promising postdoctoral fellows whose recent interaction with university research helps infuse our staff with new techniques and current knowledge.

Finally, in auditing, we use peer review. We have a group of consultants, many of whom are Nobel Prize winners, in the new areas of biotechnology with which we have no technical base or a thin technical base. We have a far more formal advisory panel that meets with us every 2 months. They know our staff through this current and frequent involvement with us and their advice is very valuable to us. It is a peer review board. The difference is that, while we value their advice and pay close attention to it, we're not required to follow it. That's an important difference.

This outside board and our contacts, our own instincts, our management experience, is what we use to decide when to quit. The decision to terminate research is probably the most single difficult and important decision management has to make. There is no formula for doing that, but there are signals when you're going up blind alleys or are deadended. Those decisions to terminate are frequently taken, and with good humor.

I think I will quit now and join my colleague here in taking your questions.

[The prepared statement of Dr. Quisenberry follows:]

GOOD MORNING, MR. CHAIRMAN, AND MEMBERS OF THE COMMITTEE ON SCIENCE AND TECHNOLOGY. MY NAME IS RICHARD QUISENBERRY. I AM DIRECTOR OF THE RESEARCH DIVISION IN THE CENTRAL RESEARCH & DEVELOPMENT DEPARTMENT OF THE DU PONT COMPANY.

I WELCOME THE INVITATION TO BE WITH YOU TODAY TO CONTRIBUTE TO YOUR CONSIDERATIONS OVER THE NEXT MONTH TO EXAMINE AND PERHAPS IMPROVE THE RATIONALE AND MECHANISMS FOR THE SUPPORT OF SCIENTIFIC RESEARCH BY GOVERNMENT. AS HISTORY SHOWS THE ROLE OF GOVERNMENT IN SUPPORTING BASIC RESEARCH IN THE UNITED STATES HAS BEEN EXTRAORDINARILY SUCCESSFUL, AND I AM CONVINCED THAT GOVERNMENT'S ROLE WILL BE AN EVEN MORE CRITICAL ONE IN THE YEARS AHEAD. AND WITHIN BASIC RESEARCH NO ISSUE IS MORE IMPORTANT - AND DIFFICULT - THAN DECIDING WHAT TO DO. THIS IS SO FOR ALL BASIC RESEARCH - THAT CARRIED OUT IN ACADEMIA AND GOVERNMENT LABORATORIES AS WELL AS IN PRIVATE SECTOR LABORATORIES SUCH AS OURS.

WHILE PROJECT SELECTION IS THE MOST CRITICAL ISSUE FOR THOSE OF US WHO MANAGE SCIENCE AND TECHNOLOGY, IT MUST BE CONSIDERED IN CONTEXT OF SEVERAL OTHER CLOSELY RELATED ACTIVITIES - ALL OF WHICH ARE A PART OF A SELECTION PROCESS. THESE ARE:

1) BUDGETING
2) STATEMENTS OF LONG-TERM OGANIZATIONAL OBJECTIVES IN TERMS OF BROAD RESEARCH THEMES WHICH CAN BE UNDERSTOOD BY THE SCIENTIFIC COMMUNITY.

3) FORMATION OF A QUALITY STAFF.
4) SELECTION OF SPECIFIC PROGRAMS OR PROJECTS.
5) SYSTEMATIC REVIEW OF PROGRESS.
6) DECISION TO CONTINUE OR TERMINATE.

I WILL TALK BRIEFLY ON EACH OF THESE ISSUES DURING THE NEXT FEW MINUTES. SINCE YOU ASKED ME TO DESCRIBE HOW DU PONT GOES ABOUT SELECTION OF BASIC RESEARCH PROJECTS, I'LL ILLUSTRATE WITH SOME EXAMPLES FROM OUR COMPANY. MY COMMENTS THIS MORNING WILL BE HIGH SPOT - ALMOST IN OUTLINE - AS YOUR STAFF HAS WISELY URGED THAT WE PLAN TO LEAVE AS MUCH TIME AS POSSIBLE FOR DISCUSSION AND QUESTIONS. I HOPE THERE WILL BE SOME - I'D LIKE TO LEARN SOMETHING FROM THE OTHER MEMBERS OF TODAY'S PANEL.

BEFORE I BEGIN, IT MAY BE HELPFUL TO SPEND A MOMENT DESCRIBING HOW DU PONT APPROACHES RESEARCH AND DEVELOPMENT IN GENERAL. OUR RESEARCH AND DEVELOPMENT BUDGET SUPPORTS ABOUT SIX THOUSAND PROFESSIONAL SCIENTISTS AND ENGINEERS WITH $1.2 BILLION FOR THE CURRENT YEAR. WE THINK OF OUR RESEARCH BUDGET AS COMPOSED OF FIVE PARTS.

BASIC RESEARCH WHICH SEEKS NEW KNOWLEDGE IN THE AREAS OF INTEREST TO THE COMPANY AMOUNTS TO ABOUT 8%. DISCOVERY AND DEVELOPMENT RESEARCH - ABOUT ANOTHER 40% - IS DESIGNED TO GET US INTO THOSE BUSINESSES. FINALLY EXTENSION AND SUPPORT ABOUT 50% OF OUR BUDGET IS AIMED AT KEEPING EXISTING BUSINESSES GOING OR - IF ITS A MATURE BUSINESS - PROVIDING SUPPORT TO RETAIN ITS

COMPETITIVE POSITION. WE FEEL PRETTY GOOD ABOUT THE ROLE THAT SCIENCE AND TECHNOLOGY PLAYED IN OUR COMPANY OVER THE PAST 50 YEARS. IN EVERY SENSE REGARD RESEARCH AND ITS TIMELY EXPLOITATION IN THE MARKET PLACE AS ABSOLUTELY CRITICAL TO OUR FUTURE.

ABOUT 85% OF OUR RESEARCH EXPENDITURES ARE IN SUPPORT OF CAREFULLY CONSIDERED AND - IN MOST CASES QUANTIFIABLE BUSINESS GOALS. SELECTION AMONG RESEARCH PROJECTS IN SUPPORT OF EXISTING BUSINESSES - WHILE USUALLY NOT EASY AND OFTEN PAINFUL - ARE DEBATED AND EVENTUALLY DONE ON A RATIONAL BASIS OF BUSINESS PRIORITY. THE SITUATION IS NOT NEARLY SO CLEAR CUT WITH REGARD TO THE 15% WHICH IS FUNDAMENTAL OR BASIC AND USUALLY INVOLVES TECHNOLOGY NOT A PART OF ANY CURRENT BUSINESS UNIT. THE MANAGEMENT CHALLENGE FOR US HERE IS QUITE ANALOGOUS - AND EVERY BIT AS DIFFICULT - AS THE QUESTION FACING THE ADMINISTRATION OF THE NATIONAL SCIENCE FOUNDATION, THE NATIONAL INSTITUTES OF HEALTH OR THE DEAN OF A LARGE GRADUATE SCHOOL IN DECIDING HOW TO ALLOCATE HIS LIMITED RESOURCES.

WE DON'T HAVE ANY SPECIAL FORMULAS, NOR DO WE USE PROPRIETARY ARCANE ANALYSES. WE DON'T CLAIM TO BE INFALLIBLE - WE MAKE MISTAKES. BUT THE FACT THAT WE ARE STILL IN BUSINESS SUGGESTS TO US THAT WE MAY BE CORRECT IN OUR DECISION MORE OFTEN THAN NOT.

OUR PROCESS BEGINS WITH BUDGETING. BUDGET PREPARATION STARTS WHEN INDIVIDUAL BUSINESS UNITS WITHIN THE COMPANY SUBMIT EXPENSE AND OTHER RESOURCE REQUIREMENTS TO ACCOMPLISH THE MISSIONS

THESE BUSINESSES HAVE BEEN ASSIGNED BY THE CORPORATION. THE MISSIONS RANGE FROM RAPID GROWTH OR - AT THE OTHER EXTREME - CASH GENERATION TO FUEL THE GROWTH OF OTHER BUSINESSES. A PROCESS OF SEVERAL MONTHS' DURATION AND MANY ITERATIONS ULTIMATELY LEADS TO A RESEARCH AND DEVELOPMENT BUDGET WHICH THE COMPANY CAN DIGEST FINANCIALLY.

THE BASIC AND DISCOVERY RESEARCH BUDGET IS NOT DEVELOPED BY THE MANAGEMENT OF THE COMPANY BUSINESS UNITS, BUT BY THE TOP CORPORATE MANAGEMENT. THE SIZE OF THE BUDGET OF THE BASIC RESEARCH BUDGET AND - IMPORTANTLY ITS SHAPE AND EMPHASIS - CONFORMS TO THE VISION OF THE FUTURE ORGANIZATION DEVELOPED BY THE CORPORATE MANAGEMENT TEAM. RATHER THAN SELECT SPECIFIC RESEARCH PROGRAMS, THE RESEARCH LEADERSHIP AND CORPORATE MANAGEMENT IN A CONTINUING DIALOG AGREE ON BROAD AREAS OF SCIENTIFIC EXPERTISE WE WISH TO ESTABLISH, AND WHICH WE BELIEVE WILL ENHANCE OUR SUCCESS IN PURSUING THE FORWARD COMMERCIAL OBJECTIVES OF DU PONT. IF WE'RE CORRECT IN IDENTIFYING THESE BROAD TECHNOLOGY THEMES, THE PAYOFF TO THE COMPANY - AND TO THE SCIENTIFIC COMMUNITY - CAN BE SIGNIFICANT.

FOR EXAMPLE, OVER THE LAST 20 YEARS WE HAVE WORKED HARD TO MAINTAIN AN EXPERTISE - TO USE OUR TERM, A CENTER OF EXCELLENCE - IN ORGANOMETALLIC CHEMISTRY. THIS IS A SPECIALIZED BRANCH OF SCIENCE WHICH HAS BECOME THE SCIENTIFIC BASIS FOR CATALYSTS, PHOTOIMAGING AND MAGNETIC RECORDING. THIS ACTIVITY IS DIRECTED BY A SCIENTIST MANAGER WHO HAS ACHIEVED SUCH PROMINENCE IN THE FIELD

THAT HE HAS SERVED AS EDITOR OF THAT DISCIPLINE'S SCIENTIFIC JOURNAL, AND LAST YEAR WAS ELECTED TO THE NATIONAL ACADEMY OF SCIENCE IN RECOGNITION OF HIS CONTINUING CONTRIBUTION TO THIS SCIENCE. FEW WOULD ARGUE THE IMPORTANCE TO DU PONT OF BEING ONE OF THE LEADERS IN THIS SCIENCE IN ORDER TO PIONEER NEW CATALYTIC CHEMICAL PROCESSES, X-RAY BASED DIAGNOSTIC SYSTEMS AND MAGNETIC RECORDING FOR AUDIO AND VIDEO COMPUTERS. WE ARE NOW DISCOVERING THAT OUR MAJOR INVESTMENT IN THIS SCIENCE HAS POSITIONED US WELL TO PARTICIPATE IN THE EMERGING BUSINESS INVOLVED IN THE TRANSMISSION AND RECORDING OF INFORMATION USING OPTICAL TECHNOLOGY.

OTHER IMPORTANT BROAD RESEARCH THEMES TO OUR COMPANY WHICH ARE REFLECTED IN OUR BASIC RESEARCH INCLUDE:

- EXPERTISE IN POLYMER SCIENCE WITH EMPHASIS ON DISCOVERY OF NEW MATERIALS, AND MORE IMPORTANTLY THE PHYSICS BY WHICH POLYMER BASED MATERIALS - OR PLASTICS - PERFORM OR FAIL IN USE.
- NEW MATERIALS BOTH ORGANIC AND INORGANIC WHICH BEHAVE IN UNIQUE AND NOVEL WAYS WHEN EXPOSED TO ELECTRICAL AND OPTICAL STIMULATION.
- UNDERSTANDING HOW PROTEINS - SUCH AS VIRUSES - INTERACT IN LIVING ORGANISMS AT THE MOLECULAR LEVEL. THE LAST PHASE IS IMPORTANT BECAUSE OUR RESEARCH OVER THE YEARS HAS TAUGHT US A GREAT DEAL ABOUT HOW SMALL MOLECULES BEHAVE, AND TECHNOLOGY IS NOW IN HAND TO EXTEND THOSE SAME PRINCIPLES TO LARGER MOLECULES IN LIVING SYSTEMS.

- GENETIC ENGINEERING TO TRANSFORM PROTEINS TO DISPLAY THE SPECIFIC, DESIRABLE TRAITS.
- NOVEL WAYS TO TREAT INDUSTRIAL BYPRODUCTS AND WASTE TO REDUCE ENVIRONMENTAL HAZARD.
- LEADING EDGE INSTRUMENTS AND NOVEL CHEMISTRY TO DIAGNOSE DISEASE.
- STRUCTURAL MATERIALS TO ENABLE THE TRANSPORTATION INDUSTRY TO ACHIEVE NEW STYLE AND FUNCTION IN AUTOMOBILES, TRUCKS AND AIRCRAFT.

THESE ARE SEVERAL OF THE RESEARCH THEMES THAT WE DISCUSS WITH OUR PROFESSIONAL STAFF, AND AROUND WHICH SPECIFIC RESEARCH PROGRAMS ARE ULTIMATELY FORMULATED AND ADVANCED. ONCE THESE BROAD THEMES HAVE BEEN UNDERSTOOD AND AGREED UPON, TWO COMMITMENTS ARE REQUIRED. THE FIRST IS BASED ON A PRINCIPLE THAT A SUCCESSFUL BASIC PROGRAM NEEDS A HORIZON OF AT LEAST TEN YEARS, AND A COMMITMENT TO THAT HORIZON. SECONDLY, SUCCESS CAN ONLY COME THROUGH A QUALITY STAFF. JUDGING THE QUALITY OF STAFF IS A DIFFICULT ISSUE. OUR EXPERIENCE IS THAT PEER REVIEW AND PROFESSIONAL STANDING ARE THE BEST WAYS TO APPROACH THE QUESTIONS. IN MOST OF OUR AREAS OF BASIC SCIENTIFIC EXPERTISE WE BELIEVE WE HAVE ATTRACTED A STAFF OF PRINCIPAL SCIENTISTS THAT COMPARE FAVORABLY WITH THE FACULTY OF ANY OF OUR LEADING RESEARCH UNIVERSITIES. WE ARE ABLE TO RECRUIT AND IMPORTANTLY TO RETAIN QUALITY SCIENTISTS BECAUSE - FRANKLY - WE THINK WE HAVE AN ATTRACTIVE ENVIRONMENT IN WHICH TO DO GOOD SCIENCE. WE STRESS

STAFF QUALITY BECAUSE IN BASIC PROGRAMS THE SPECIFIC RESEARCH PROJECTS ARE GENERATED BY OUR PRINCIPAL SCIENTISTS, OR INCREASINGLY, TEAMS OF SCIENTISTS. WE BELIEVE WE COMMIT GENEROUS SUPPORT RESOURCES TO BASIC RESEARCH PROGRAMS AND - MOST IMPORTANTLY - KEEP ADMINISTRATIVE BURDENS AND PAPERWORK TO A MINIMUM. WELL OVER HALF OF OUR STAFF COME FROM UNIVERSITY APPOINTMENTS OR FACULTY POSITIONS. THEY COMMENT ON HOW EASY IT IS TO PROPOSE AND "SELL" A RESEARCH PROJECT IN OUR LABS NOT GO THROUGH THE TORTUROUS GRANT PREPARATION PROCESS THAT HAS BECOME THE NORM IN ACADEMIC LABS.

WE ARE COMMITTED TO THE CONCEPT OF RESEARCH PROJECTS GENERATED BY THE SCIENTIFIC STAFF RATHER THAN FORMULATED AT THE TOP AND "PASSED DOWN" BY MANAGEMENT. OUR REASON FOR THIS VIEW IS SIMPLE. MOST IMPORTANT SCIENTIFIC DISCOVERIES AND IMPORTANT NEW PRINCIPLES ARE MADE FOLLOWING LINES OF INVESTIGATION PERIPHERAL TO THE ARTICULATED OBJECTIVE OF THE PROJECT. SIMPLY PUT, INNOVATION CANNOT BE FORECAST. OUR EXPERIENCE CONVINCES US THAT THE COMBINATION OF QUALITY STAFF TEAM PURSUING A PROGRAM TO WHICH IT IS BOTH INTELLIGENTLY AND EMOTIONALLY COMMITTED WILL RESULT IN FUNDAMENTALLY USEFUL NEW KNOWLEDGE AND THOSE OCCASIONAL BREAKTHROUGHS THAT ARE SO EXCITING AND MAKES THE OFTEN FRUSTRATING WORLD OF RESEARCH SO THRILLING.

ANOTHER IMPORTANT ELEMENT IN ENSURING SUCCESS IN BASIC RESEARCH, WE THINK, IS THE INTERACTION BETWEEN OUR SCIENTISTS AND THEIR COLLEAGUES. WE ENCOURAGE STRONGLY COLLABORATIVE

INTERACTIONS WITH LEADING SCIENTISTS IN UNIVERSITIES AND GOVERNMENT LABORATORIES AND IN SOME CASES COUNTERPARTS IN OTHER INDUSTRIAL LABORATORIES. WE ALSO HAVE A PROGRAM TO BRING TO DU PONT - FOR A STAY OF A YEAR OR TWO - LEADING ACADEMIC SCIENTISTS ON SABBATICAL LEAVE OR PROMISING POSTDOCTORAL FELLOWS WHOSE RECENT INTERACTION WITH UNIVERSITY RESEARCH HELPS INFUSE IN OUR STAFF CURRENT TECHNIQUES AND KNOWLEDGE.

TURNING TO AUDITING, WE EVALUATE THE PROGRESS OF OUR PROJECTS AND, IN FACT AUDIT, OUR JUDGMENT IN SELECTING THEM IN THE FIRST PLACE IN A NUMBER OF WAYS. FIRST WE HAVE PERIODIC MANAGEMENT REVIEWS AND RELY ON INTERNAL SCIENTIST MANAGEMENT TO EVALUATE RESULTS. WE RELY ON A HIGH-QUALITY GROUP OF CONSULTANTS INCLUDING SEVERAL NOBEL PRIZE WINNERS TO ASSIST MANAGEMENT AND THE SCIENTISTS THEMSELVES IN MONITORING THE PROGRESS OF RESEARCH. CONSULTANTS HAVE LONG APPOINTMENTS WITH US AND BRING A DESIRABLE LEVEL OF CONTINUITY.

WE DON'T HAVE DEPTH IN THE SCIENCE MANAGEMENT IN SOME FIELDS SUCH AS BIOTECHNOLOGY AND SOME AREAS OF ELECTRONICS. IN THESE, WE RELY ON PANELS OR ADVISORY BOARDS WHO VISIT SEVERAL TIMES A YEAR AND CARRY OUT A PROCESS QUITE SIMILAR TO THE SITE VISIT AND PEER REVIEW BOARDS COMMON IN ACADEMIC LABORATORIES. A DIFFERENCE IS THAT THE SITE VISITING COMMITTEE COMES EVERY SEVERA MONTHS AND THUS DEVELOPS IN-DEPTH KNOWLEDGE OF OUR STAFF AND PROGRAMS. WE THINK THEY PROVIDE EXCEPTIONALLY USEFUL ADVICE. WE DON'T TAKE ALL OF THEIR ADVICE BUT ALWAYS GIVE IT CAREFUL CONSIDERATION.

THESE OUTSIDE PEER REVIEW MECHANISMS ARE PARTICULARLY USEFUL IN CONFIRMING THE JUDGMENT OF DU PONT MANAGEMENT WHEN WE CONSIDER TERMINATING A LINE OF RESEARCH. THE DECISION TO STOP IS THE MOST DIFFICULT DECISION FOR RESEARCH MANAGEMENT AND WE HAVE DISCOVERED NO GOOD FORMULA FOR MAKING IT. THERE ARE SIGNALS WHEN A RESEARCH PROGRAM IS DEAD-ENDED OR GOING UP A BLIND ALLEY. THE DECISION TO TERMINATE IS TAKEN AFTER VERY CONSIDERABLE THOUGHT AND INVOLVES AGREEMENT OF THE RESEARCH TEAM WHO HAVE RESPONSIBILITY FOR THE PROGRAM.

MR. CHAIRMAN, I OUTLINED SOME OF THE ELEMENTS OF OUR APPROACH TO PROJECT SELECTION AND MANAGEMENT. IN VIEW OF TIME CONSTRAINTS, MY DESCRIPTION HAS BEEN SUPERFICIAL. I AM PREPARED AND EAGER TO AMPLIFY ANY OF THESE POINTS TO WHATEVER DETAIL YOU MAY WISH. THANK YOU.

RKQ/SMG

Mr. **Packard**. Thank you very much, Dr. Quisenberry.

Again, we have almost a frivolous vote to make, but I think we need to go over and make it, and we only have about 6 minutes to go and do that. So we will do that and then address questions, if we could impose upon you to remain.

[Whereupon, the task force was in short recess.]

PANEL 2 DISCUSSION

Mr. **Packard**. We'll call the hearing back to order. I appreciate you gentlemen being patient with us.

Again, this is an opportunity for us to perhaps direct our questions a little bit differently, inasmuch as we're dealing with industry.

I was interested in your comment, Dr. Quisenberry, where you have presented a rather attractive picture of basic research in the corporate setting. I would like to pick your brain a little bit in terms of the basic research versus applied research, which we have had considerable discussion on, not only in this series of hearings in the last 3 days but in many other hearings. Recognizing that this country has lagged behind in the application of basic research as it relates to at least putting a product on the market and in the competitive world, there is the general feeling that we have not done a good job of taking our basic science and applying it to a marketable product as well as other places in the world.

There at Du Pont, do you do a lot of basic research that is not funded by Federal funds?

Dr. **Quisenberry**. We do no basic research funded by Federal funds.

Mr. **Packard**. None at all by Federal funds?

Dr. **Quisenberry**. No.

Mr. **Packard**. And IBM?

Dr. **Armstrong**. Almost none.

Mr. **Packard**. Almost none. But you do a considerable amount of basic research?

Dr. **Quisenberry**. Yes.

Dr. **Armstrong**. Yes, essentially all funded by the corporation.

Mr. **Packard**. By the corporation.

Do you find that in the research that you do without Federal involvement you have a better track record of moving that research to a marketable product?

Dr. **Quisenberry**. I think we have a good track record of moving basic research into application and ultimately to the marketplace. I think the reason for that is not so much associated with whether it's federally funded or not. It goes to the question of technology transfer. I think one of the problems that we're really talking about, when we hear comments as we did this morning about the problem is the scaleup of basic research, is really the question of technology transfer. In an organization such as ours—and I'm sure my colleague here from IBM would say the same thing—technology transfer is far more easy than it is between an academic or a Government laboratory and the private sector, and the reason for that is simple. You transfer technology with people and not with documents and reports.

Dr. **Armstrong**. Could I add something to that?

Mr. **Packard**. Please do.

Dr. **Armstrong**. I would say that I would agree with my colleague, that the structure of an industrial setting makes it easier to do technology transfer. I would also say that technology transfer from basic research is one of the hardest things we have to learn how to do. A major portion of the management attention and time, of the senior management of a laboratory doing basic research, has to be devoted to technology transfer and to being effective at that, to investing new mechanisms to do it.

Over the last few years in IBM research, we have invented several new mechanisms to address that issue, because that's a key.

Mr. **Packard**. The thrust of mv question was to try to determine if, in fact, federally funded basic research is a disincentive or is acting as a disincentive. I gather from your comments that that's not the case; that, in fact, there are other reasons perhaps.

Dr. **Quisenberry**. Not at all. I don't think that's an issue.

Dr. **Armstrong**. Right.

Mr. **Packard**. Thank you very much.

We not only have a vote now on recall, but we will have a series of votes. We knew that before we came back. But we thought we would maybe be able to get several questions in before and then adjourn.

I would like to ask Mr. Brown to ask a question or two, if he wishes, and we'll go on for another 5 minutes. Then we'll need to go. Also, I think we will be detained for several votes in a row, so we will adjourn the hearing before we leave this time and then submit questions if we desire, to be responded to in writing, if you would not object.

Mr. **Brown**. That's in consideration of your time, gentlemen.

Mr. **Packard**. It is.

Mr. **Brown**. We don't want to keep you here unnecessarily.

Mr. **Packard**. Mr. Brown, do you have questions?

Mr. **Brown**. Yes. Let me get to a point we were talking about with the previous witnesses, although I know it is not the thrust of your own report, but fundamentally the role that the Government should play in this process of moving fundamental knowledge into a utilitarian status of some kind or other.

I was struck, Dr. Armstrong, by your statement, where you said university scientists think we know all there is to know about S_iO_2, and you think and your organization thinks that we all know almost nothing. It seems to me the reason for that is quite simple. Most university scientists looking at S_iO_2,think of it as sand, and you think of it as an essential ingredient.

Dr. **Armstrong**. I think of it as a one megabit memory, too.

Mr. **Brown**. Right. And that may be the difference there. You're thinking of an ultimate utilization.

Dr. **Armstrong**. And, in addition, that utilization has the property that it involves S_iO_2,under what really are extreme conditions, namely, a layer a hundred angstroms thick and a square centimeter or so.

Mr. **Brown**. Of course, the role of the Government is obviously handicapped in their selecting of, let's say, targeted-basic research, all the way from targeted-basic up to commercialization technology, because they don't have a concept at this point and no way to develop a concept of what's going on.

The importance of the role the National Science Foundation plays in supporting fundamental research is just the fact, that by filling in the gaps and making sure we have a strong, across-the-board capability in basic research and the enhancement of knowledge, it's going to benefit in a general way all of those who are seeking to utilize that knowledge for specific purposes. But when we get beyond that, we're lost.

The question is, how do we keep from getting lost? How do we look at the broader national goals and develop a sense of what needs to be done that will contribute to those goals without infringing upon the role of the private sector and yet build on the basic research we have created?

Dr. Armstrong. Well, that's a long and very important——

Mr. Brown. In a minute or less. [Laughter.]

Dr. Armstrong. I served for a number of years as Chairman of the Advisory Committee for Physics of the NSF, so I know something about that organization. I'm going up to Harvard tomorrow to give them a talk on what science we ought to do to keep the sort of computer revolution going.

There may be institutional improvements that we can make and that the Federal Government can mediate and help with this process of transfer. There may be. Although coming from the private sector, I tend to think that's something that we will understand how to do most effectively.

But I am very concerned, that quite apart from the utilization of basic research—and the interaction between industry and universities is one of our best hopes there—I tell my colleagues that they don't know about S_iO_2,when I go up there. But one of the things that the Federal Government can do, I think, is to make sure that we do not slip back in the rate at which we produce that fundamental knowledge. Because, remember I said we have this gap between scientific knowledge and technology, and it's not closing. The computer industry is going like crazy and using up scientific knowledge. So there is a set of issues as to how we utilize the basic knowledge, but there is also a serious issue that the storehouse is not being replenished constantly.

Mr. Brown. I think that's right, and maybe that's the most important thing we can do at this point.

This administration, to its credit, recognized the great importance to supporting basic research, fundamental research. Yet, despite that recognition, the overall level of Federal resources applied to the support of civilian basic research is level. It is not growing at all. The emphasis on quality is good, but the main growth in the R&D budget over the last 5 years has been in fundamentally military applications.

Dr. Armstrong. Let me just say that from the experience in doing large commercial scaleup projects, let's say, in IBM, we often come to grief because of a lack of basic materials knowledge. We have to go back and fix that, and then all is well. But these big development projects that proceed and spend huge amounts of money, without the basic knowledge, will be inefficient for that reason.

Mr. **Brown**. The Congress has sensed that something needed to be done here for many years. One of the things we tried to do was to try and distort the role of the National Science Foundation, to put more into what we call RAND, Research Applied to the National Needs, and other things. It didn't work very well, partly because I think there was a shortage of overall money and the money that went into that came out of support for fundamental research.

But today, NSF is doing a good job in recognizing this materials research needs, for example. It's setting up Engineering Research Centers. It's moving in incremental steps in a very constructive way. Again, I think rather fearfully, what they're doing may lead to a curtailment of the support that they can give to the individual initiated fundamental research.

Mr. **Packard**. I think we had better call the time. We have just enough time to get over to vote. I wish we had time to pursue this. This has been very stimulating.

We sincerely appreciate you being with us, Dr. Armstrong and Dr. Quisenberry. Your testimony has been excellent and we appreciate your willingness to come.

We will adjourn this hearing then and quickly go and vote.

[Whereupon, at 11:43 a.m., the task force was adjourned to reconvene on Tuesday, April 15, 1986.]

APPENDIX 1—ADDITIONAL MATERIALS SUBMITTED FOR THE RECORD BY DR. RUTH L. KIRSCHSTEIN

Decisions by Director, NIH, on Recommendations of Grants Peer Review Study Team

February 8, 1978

U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service
National Institutes of Health

February 8, 1978

SUMMARY OF DECISIONS BY DIRECTOR, NIH ON RECOMMENDATIONS OF GRANTS PEER REVIEW STUDY TEAM

Dr. Donald S. Fredrickson, Director of the National Institutes of Health, announced today that he has approved 33 of the recommendations submitted to him by the NIH Grants Peer Review Study Team for the improvement of the Agency's Grants Review Process. Additionally, nine of the Team's proposals were approved with minor modifications.

The NIH Deputy Director, Dr. Thomas E. Malone, was given the responsibility to carry out the approved recommendations and to arrange for further studies of 19 of the proposals deferred pending additional examination and discussion. Three of the Study Team's recommendations were rejected and five required no action by the Director.

The 12-member NIH Grants Peer Review Study Team, headed by Dr. Ruth L. Kirschstein, Director of the National Institute of General Medical Sciences, conducted a searching review of the system used by the National Institutes of Health for evaluating the more than 15,000 research and training proposals the Agency receives each year.

In the course of the study, the Team held public hearings in Chicago, San Francisco and Bethesda and actively sought written comments from the present and former non-Federal members of review bodies, as well as from applicants, grantee institutions and the general public.

One set of the recommendations approved by Dr. Fredrickson was designed to improve communication with applicants, and would require NIH Advisory Councils and Boards to provide all applicants with the complete Summary Statements or critiques of their proposals, including assigned priority scores, promptly after final action is taken on them.

Another cluster of the proposals adopted by the Director is aimed at opening the process for nominating and selecting non-Federal advisors for service on councils and initial review groups. It calls for widespread publication of information about the peer review system itself with periodic general invitations for nominations of potential council or review group members. A central list of nominees will be maintained for referral to the various advisory groups as replacements for members whose terms expire.

The approved recommendations would also require that the membership of special review groups be reported to the applicant whose proposal they review and that the names and qualifications of all appointed council members be published in the NIH Guide to Grants and Contracts.

Several of the recommendations approved by Dr. Fredrickson prescribed means for tightening procedures to avoid apparent or real conflicts of interest involving members of review groups.

Dr. Fredrickson withheld his approval pending further study of a group of recommendations to establish a formal grants peer review appeals system including the appointment of an ombudsman. In deferring his decision on implementation of the appeals system, the Director noted that setting up such a system would entail a number of legal, financial and personnel resource questions which must be further evaluated.

The Study Team had recommended that applicants be required to identify proposals they consider to be "especially innovative" and to support that contention in detail. In the same vein, the Team also proposed that NIH Initial Review Groups be encouraged to prepare special statements regarding "innovative aspects" of proposals, and to flag such proposals for special consideration. Both recommendations were rejected by Dr. Fredrickson on the ground that researchers generally consider their work to be innovative and that review groups especially seek this quality in their analyses. Rather than create a special class of proposals, he recommends that review group members be encouraged and reminded to give due weight to "innovative" aspects of all applications in setting their priority scores.

Dr. Fredrickson also rejected the recommendation that NIH seek to have the authority for appointments to advisory councils and boards delegated from the Secretary, HEW, to the Assistant Secretary for Health.

Following the submission of the GPRST Report and Recommendations to the Director in January 1977, he appointed a small working group of senior staff members and program heads to examine each recommendation in the light of its legal and operational implications, and to communicate with each Bureau, Institute and Division regarding the GPRST proposals. The working group assembled BID comments on the recommendations, and again circulated them for further comment. The final BID comments and consensus of the members of the working group together with proposals for action were then submitted to the Director for his decisions.

Attached is a listing of the Study Team's 69 recommendations together with the Director's decisions and comments regarding them.

Attachment

DECISIONS BY DIRECTOR, NIH ON RECOMMENDATIONS OF GRANTS PEER REVIEW STUDY TEAM

Recommendations #1 and #9A were considered together:

RECOMMENDATION #1 -- *(#1 page 81 of the GPRST Report)*

That, in order to make the reviewer selection process better and more widely understood and to regularize the ways in which the scientific and lay communities contribute information in support of it, the NIH should publish periodically (e.g., annually) an announcement which calls attention to the upcoming vacancies on IRGs associated with the Grants Peer Review System and which invites suggestions regarding candidates for specific IRGs.

RECOMMENDATION #9A -- *(#2 page 94 of the GPRST Report)*

That the procedure for nominating and selecting Council members be more open. Scheduled Council vacancies should be announced and published early in the fiscal year. The announcement should specify the Council involved, the criteria for the selection of Council members, their duties, and terms of appointment.

DECISION

The process of selecting reviewers for NIH grant proposals should be open. In order to broaden the approach recommended by the Study Team we will develop a reservoir of individuals qualified to serve on advisory groups by:

- publishing a description of the NIH Peer Review System as an issue of the NIH Guide to Grants and Contracts, as well as in at least one widely read scientific journal.
- publishing periodically descriptions of the Peer Review System, listing current advisory groups, detailing the selection process, and inviting nominations of potential members. Such nominations will be collected centrally and subsequently referred to the appropriate advisory group. In this way the NIH will develop a reservoir of potential consultants from which replacements may be selected for those reviewers whose terms are expiring. Every effort must be made to seek scientists who are women, ethnic minorities and of a younger age and have skills consistent with the demanding standards of the review process.

RECOMMENDATION #2 -- (#2 page 82 of the GPRST Report)

That, in order to provide for additional advice beyond that given by Initial Reveiw Group members, particularly in regard to those research grant applications in which new scientific fields are to be explored or unique procedures are to be developed, the NIH should implement a formal procedure whereby an applicant may identify those particular aspects of the proposed research which are considered to be unique and to suggest possible consultants who are considered to be leaders in and currently knowledgeable about his/her area of research. However, the decision as to whether to seek advice from the suggested expert(s) or any other expert(s) must remain that of the NIH.

DECISION

It is recognized that most applicant-investigators consider their research to be unique. However, the recommendation that applicants may suggest their own reviewers could result in the creation of apparent or real conflicts of interest whose avoidance would add special administrative problems to an already overburdened system.

Special efforts must be made by reviewers and staff to identify unique or unorthodox research. The need to be alert to unique ideas should be stressed in the orientation of new members of initial review groups and councils and in the Guide for new members. Executive Secretaries and staff should be especially attentive to this need.

RECOMMENDATION #3 -- (#3 page 83 of the GPRST Report)

That the current practice of consultation with present, retiring, and former IRG members concerning possible new members should continue to be but one of a series of steps by which advice concerning potential IRG members is sought from many sources.

DECISION

Concur.

RECOMMENDATION #4 -- (#4 page 83 of the GPRST Report)

That the current practice of providing only one nomination for each IRG vacancy should be retained. Furthermore, advice regarding such nominations to DRG Initial Review Groups should be sought from knowledgeable and interested staffs of the various BIDs responsible for the administration of grants in the particular biomedical scientific areas.

DECISION

Concur.

RECOMMENDATION # 5 -- (#1 on page 88 of the GPRST Report)

That it is essential that NIH continue to have the flexibility and opportunity to establish special Initial Review Groups.

DECISION

Concur.

RECOMMENDATION #6 -- (#2 on page 88 of the GPRST Report)

That NIH and HEW should oppose legislation regarding ad hoc groups.

DECISION

Concur.

RECOMMENDATION #7 -- (#3 on page 89 of the GPRST Report)

That the roster of consultants to be included on a special Initial Reveiw Group be submitted to all investigators.

DECISION

All consultants serving in a Special Initial Review Group should be listed in the Summary Statement transmitted to the applicant-investigator. (See Rec. #53 below.)

RECOMMENDATION #8 -- (#1 page 94 of the GPRST Report)

That NIH should seek to have the authority for selection and appointment of members of Advisory Councils/Boards delegated to the Assistant Secretary for Health, DHEW.

DECISION

I believe that it is appropriate for the Secretary of Health, Education, and Welfare to retain authority to select and appoint members of Advisory Councils. Therefore I do not concur with the Study Team's recommendation.

RECOMMENDATION #9A was considered with Recommendation 1, page 1.

RECOMMENDATION #9B -- (#2 page 94 of the GPRST Report)

The names and qualifications of the selected Council members should be published.

DECISION

I concur and suggest that the names and qualifications of selected Council members be published in the NIH Guide to Grants and Contracts.

RECOMMENDATION #10 -- (#3 page 95 of the GPRST Report)

That, when a selection has been made for a Council vacancy(ies) other than from nominations submitted by the Director, NIH, the appointment should not be made final until the Director, NIH, has had the opportunity to comment on the selection.

DECISION

Concur.

RECOMMENDATION #11 -- (#1 page 100 of the GPRST Report)

That NIH develop detailed supplemental instructions to be sent to members along with the Form HEW 474, emphasizing that employment includes, for example, foreign employment, summer and part-time employment, membership on advisory boards of organizations, and consultant appointments; and that relevant financial interests include such things as royalty agreements with or stock ownership in domestic or foreign biomedical research organizations, companies engaged primarily in providing service to such organizations, pharmaceutical companies, and other companies involved in drug research.

DECISION

Concur.

RECOMMENDATION #12 -- (#2 page 101 of the GPRST Report)

That NIH adopt a procedure under which Form HEW 474 is returned to a member as incomplete, where such member makes no entry in a section of the form but does not write "none" or some equivalent in the section.

DECISION

Concur.

The following three recommendations were considered together:

RECOMMENDATION #13 -- (#3 page 101 of the GPRST Report)

That the Executive Secretaries of IRGs be given access to the Forms HEW 474 of members, as needed.

RECOMMENDATION #14 -- (#4 page 101 of the GPRST Report)

That review of Forms HEW 474, at least annually, by each Executive Secretary be mandated throughout NIH.

RECOMMENDATION #15 -- (#5 page 101 of the GPRST Report)

That all Executive Secretaries be required periodically to attend training sessions on evaluation of conflict-of-interest situations.

DECISION

At this time the recommended procedures are carried out in some components of NIH and not in others. Current practices should be reviewed and uniform procedures be developed covering all members of initial review groups and advisory councils.

RECOMMENDATION #16 -- (#1 page 102 of the GPRST Report)

That all initial invitations to serve on advisory committees make it explicit that final appointment is contingent upon review of the completed Form HEW 474 for conflicts of interest, or potential conflicts of interest, and that the new member be formally notified of the appointment after the appointment process (including review of the Form HEW 474) has been completed.

DECISION

Concur.

RECOMMENDATION #17 -- (#2 page 102 of the GPRST Report)

That the names of new members of advisory committees not be released to the news media or other members of the public until such time as final appointment has occurred after completion of the Forms HEW 474.

DECISION

Concur.

RECOMMENDATION #18 -- ((#III page 105 of the GPRST Report)

That employees of "for-profit" organizations be eligible for membership on all Initial Reveiw Groups considering grant applications (including National Research Service Award applications), and that the selection of these individuals be based on the scientific proficiency necessary to fill the various needs of specific committees. The basis for selection of such scientists shall be the same as for those employed by "non-profit" organizations.

DECISION

Current NIH practices are in accord with the Study Team's recommendation. No further action is required.

RECOMMENDATION #19 -- (#III page 105 of the GPRST Report)

That the conflict of interest statements (HEW Form 474)submitted by employees of profit-making organizations nominated as consultants must be reviewed by the agency head, who must be satisfied that each nominee can serve on the specific committee for which such person has been proposed without being in violation of the conflict of interest statutes.

DECISION

Concur.

RECOMMENDATION #20 -- (#IV page 105 of the GPRST Report)

The criteria for selection of IRG members from "for-profit" organizations should be no different than those for selection of persons from "non-profit" organizations.

DECISION

No action required. See Rec. #18 above.

RECOMMENDATION #21 -- (Page 116 of the GPRST Report)

That the PHS Act be amended to provide statutory exemption from the requirements of the Freedom of Information Act for disclosure of research designs and protocols presented in grant applications.

DECISION

Concur.

RECOMMENDATION #22 -- (Page 119 of the GPRST Report)

That NIH and HEW present specific written opposition to any legislative proposal extending the Federal Advisory Committee Act to ad hoc or Special Review Groups, based on the need to prevent conflicts of interest in the peer review of grant applications and the need to retain flexibility in meeting short-time workload increases in specialized areas.

DECISION

The amendments to the Federal Advisory Committee Act introduced in the 95th Congress do not contain any provision relating to ad hoc or special review groups. No action is required at this time. However, the NIH should oppose any such legislative proposal when offered.

Recommendations 23 and 53 were considered together:

RECOMMENDATION #23 -- (Page 123 of the GPRST Report)

That the Summary Statements be made available upon request to the principal investigators, including the priority scores.

RECOMMENDATION #53 -- (#1 page 178 of the GPRST Report)

That, as soon as practical after a BID's National Advisory Council/Board completes the review of a grant application, the BID should routinely send the Summary Statement, with the priority score displayed, to the principal investigator. Because Summary Statements frequently contain forthright and critical comments concerning the principal investigators, this recommendation is made contingent upon the understanding that these documents will be released by NIH only to these individuals. If it is determined that such NIH-initiated release of Summary Statements could reasonably lead to a requirement that NIH make these documents available also to applicant institutions or others, then the study team recommends that Summary Statements with priority scores displayed be released only under Privacy Act procedures, i.e., upon receipt of an appropriate request from the individual concerned.

DECISION

Recommendation 23 (See also Decisions regarding Recs. #53 and #54)

Concur with recommendation to include priority score with Summary Statements sent to Principal Investigator subsequent to final action by National Advisory Council/Board. When Summary

Statements are provided to Principal Investigators prior to final action, priority score will be deleted.

DECISION: Recommendation #53

Concur, with the condition that an explanation be transmitted with each Summary Statement concerning the significance of raw and normalized scores as a substantial element, but not the sole determinant of the funding decision.

RECOMMENDATION #24 -- (Page 123 of the GPRST Report)

That in releasing reviewers' opinions under the Privacy Act, an adequate legal basis be established for protecting the reviewers' anomymity either through reinterpretation of existing law, if reasonably possible, or enactment of new legislation.

DECISION

Concur.

RECOMMENDATION #25 -- (Page 124 of the GPRST Report)

That those portions of the meetings of advisory groups which involve the review of grant applications continue to be closed to the public (including those submitting applications), either under current exemptions to the open-meeting requirement or through a statutory amendment.

DECISION

Concur.

RECOMMENDATION #26 -- (Page 124 of the GPRST Report)

That, notwithstanding proposals which would require public representation on all committees, the current system of dual review be preserved, with grant applications being reviewed first by Initial Review Groups consisting solely of scientific and technical experts and then by National Advisory Councils and Boards which include representatives of the public.

DECISION

Concur.

RECOMMENDATION #27 -- (Page 125 of the GPRST Report)

That the NIH and HEW establish a mechanism for special, periodic assessment of the impact of this new legislation on the quality of grant applications and on the quality of peer review of such applications and that such assessment be reported to the legislative and executive branches of the government.

(This recommendation concerns the Privacy Act, the amended Freedom of Information Act and the Federal Advisory Committee Act.)

DECISION

I concur in principle with the Study Team's recommendation that assessment be made of the effect of the "Sunshine laws" on the quality of applications and their review. However, implementation of the recommendation will require first the development of acceptable study protocols.

RECOMMENDATION #28 -- (#1 page 132 of the GPRST Report)

That the Director, NIH, take immediate steps to limit the workload of all Initial Grant Review Groups to a level which is compatible with maintaining the high quality of peer review.

DECISION

Concur.

RECOMMENDATION #29 -- (#2 page 132 of the GPRST Report)

That authority to establish or discontinue Initial Grant Review Groups, as the peer review workload dictates, should be delegated to the Director, NIH.

DECISION

Concur.

RECOMMENDATION #30 -- (#3 page 132 of the GPRST Report)

That additional resources be provided for peer review of grant applications where acceptable alternative approaches to reduction of workload will not permanently and effectively resolve long-standing review overloads.

DECISION

Concur.

RECOMMENDATION #31 -- (#4 page 133 of the GPRST Report)

That the Director, NIH, establish a permanent mechanism to determine and ensure an appropriate ceiling or maximum workload for each NIH Initial Grant Review Group.

DECISION

Concur.

RECOMMENDATION #32 -- (#5 page 133 of the GPRST Report)

That the NIH consider whether there are peer review services to NIH staff, advisory councils, or applicants which might be curtailed if all other workload reduction alternatives should fail.

DECISION

Concur.

RECOMMENDATION #33 -- (#1 page 146 of the GPRST Report)

That the proposed peer review regulations be finalized as soon as possible.

DECISION

Proposed peer review regulations have been submitted for Departmental review and when approved will be published in the Federal Register.

RECOMMENDATION #34 -- (#2 page 146 of the GPRST Report)

That NIH standards and guidelines be prepared or revised as soon as possible after consideration and evaluation of recommendations made in the study by the Executive Secretaries Review Activities Committee.

DECISION

The Executive Secretaries Review Advisory Committee is conducting a study of all policies and procedures developed by NIH and its Bureaus, Institutes and Divisions for the review of grant applications. The committee has been asked to recommend agency-wide guidelines and standards. I concur with the Study Team recommendation regarding early consideration of such committee recommendations.

RECOMMENDATION #35 -- *(#3 page 146 of the GPRST Report)*

That, in order to maintain and improve the level of excellence of the grants peer review system at NIH, a personnel position should be established within the Office of Extramural Research and Training to provide for quality assurance of the system. Such a person should have staff responsibility for matters pertaining to the formulation, development, interpretation, and application of policies relating to the assignment and overall scientific and technical review of research grants and manpower development programs.

DECISION

The Study Team correctly recognized the importance of the function described. However, most of them are now part of the responsibility of the Office of the Associate Director for Scientific Review, DRG. Prior to a decision about establishing a new position further discussion and examination is necessary.

RECOMMENDATION #36 -- *(#4 page 146 of the GPRST Report)*

That training curricula be developed by NIH for extramural program and review staff in order to provide orientation and to refresh and reiterate principles concerning the philosophy, objectives, and procedures for peer review.

DECISION

Concur.

RECOMMENDATION #37 -- *(#5 page 146 of the GPRST Report)*

That open forums and workshops for program and review staff be established on a continuing basis so as to encourage and improve exchange and communication of ideas concerning issues relevant to the peer review system.

DECISION

Concur.

RECOMMENDATION #38 -- *(#6 page 146 of the GPRST Report)*

That the Director, NIH, the BID Directors, and their staffs hold orientation sessions annually for all IRG and Council members, particularly new members, in order to place peer review in perspective and to inform reviewers of their functions, duties, and responsibilities.

DECISION

Adequate orientation of IRG and council members is essential. I have adopted the following alternative to the Study Group's recommendation to accomplish that purpose and at the same time conserve time and effort of NIH officials and advisors -- ("That the responsible officials and staffs of the various initial review groups or Advisory Councils/Boards hold annual orientation sessions for their new members in order to place peer review in perspective and to inform reviewers of their functions, duties and responsibilities.")

RECOMMENDATION #39 -- (#A page 160 of the GPRST Report)

That it be recognized as an NIH policy position that the use of business management consultants as reviewers is a necessary adjunct to the scientific review of the large, complex and multifaceted program projects, centers, or those grants involving consortium arrangements.

DECISION

Concur.

RECOMMENDATION #40 -- (#B page 160 of the GPRST Report)

That the principal criteria used to determine the need for business management consultant support be related to the organizational or administrative complexity existing in any particular grant application.

DECISION

Concur.

RECOMMENDATION #41 -- (#C page 160 of the GPRST Report)

That, considering the nature of the traditional investigator-initiated research project application and its review, it be recognized that the use of business management consultants would not usually be considered necessary or appropriate.

DECISION

Concur.

RECOMMENDATION #42 -- (#D page 161 of the GPRST Report)

That NIH develop a policy issuance concerning the role of and need for assessment of business management in the review of the large program project and center grant applications.

DECISION

Concur.

RECOMMENDATION #43 -- (#E page 161 of the GPRST Report)

That NIH develop uniform guidelines to be followed by the business management consultants in their roles as members of project site visit teams and advisory groups.

DECISION

Concur.

The following two recommendations were considered together:

RECOMMENDATION #44 -- (#F page 163 of the GPRST Report)

That business management consultants should contribute to over-all recommendations on project site visits and at Initial Review Group meetings, but they should not vote on "approval vs disapproval" or give a priority rating unless, in the judgment of the Executive Secretary, the type of project includes significant non-scientific aspects clearly within their professional expertise.

RECOMMENDATION #45 -- (#G page 163 of the GPRST Report)

That business management consultants should prepare a specific portion of project site visit reports and/or IRG Summary Statements which should be recognized as separate from scientific review and evaluation, but should be carefully considered in reaching recommendations for each project.

DECISION

The issues raised by these recommendations have been discussed at length and inconclusively by Institute Directors and other reviewers of the Study Team report. Action will be deferred on these recommendations pending further examination and discussion.

RECOMMENDATION #46 -- (#H page 164 of the GPRST Report)

That procedures be developed which would allow the reports of the business management consultants, including specific recommendations for management improvement, to be made available to the applicant institution by the BID at an appropriate time after completion of the review.

DECISION

Reports of business management consultants, including specific recommendations for management practices germane to the application under consideration should be incorporated in the Summary Statement.

RECOMMENDATION #47 -- (#I page 164 of the GPRST Report)

That grants management officers of the BIDs participate with the IRG Executive Secretaries in the determination of a need for, and in selection of, business management consultants for the project site visit team involvement.

DECISION

Concur.

RECOMMENDATION #48 -- (#J page 165 of the GPRST Report)

That, whenever possible, BID grants management staff members should accompany the site visit teams on the reviews of the complex projects, serving as staff resources only.

DECISION

Concur, but amended as follows--"That, whenever appropriate, BID grants management staff members should accompany the site visit teams on the reviews of complex projects serving as staff resources only."

RECOMMENDATION #49 -- (#1 page 171 of the GPRST Report)

That NIH require the applicant to identify and support in detail the contention that the research project being proposed is especially innovative.

DECISION

This recommendation places an unnecessary obligation on the applicant-investigator who would be required to amplify justification of his proposal as "especially innovative" or tacitly accept its designation as routine or pedestrian. The peer review system must remain alert to innovation. (See Rec. #2 above). The Study Team's purpose in making this recommendation is understood, but I do not believe its implementation would be productive and therefore do not concur with it. The approach proposed in recommendation 50 appears to be much sounder.

RECOMMENDATION #50 -- (#2 page 171 of the GPRST Report)

That Initial Review Groups be requested to identify applications they consider to be especially creative or innovative, whether or not the applications were so identified by the applicant.

DECISION

Concur.

RECOMMENDATION #51 -- (#3 page 171 of the GPRST Report)

That IRG members (as a group or individually) be encouraged to prepare a statement in addition to the regular Summary Statement pointing out the innovative aspects of the application and its significance. This statement is to be prepared regardless of whether the application was recommended for approval or disapproval; these applications will be flagged for special consideration by BID ~~staff~~ and/or Advisory Council.

DECISION

The effect of this recommendation would be to create a separate class of proposals, to the possible disadvantage of the applicant. Instead of preparing a special report on "innovative" proposals, Initial Review Group members should identify, evaluate and call attention to innovative aspects of an application. As in the case of recommendation 49 I do not believe the implementation of recommendation 51 would be productive, and therefore do not concur with it.

RECOMMENDATION #52 -- (#4 page 171 of the GPRST Report)

That NIH consider the feasibility of developing an experiment involving limited support for certain speculative, high-risk, innovative research proposals. Such a study might be part of a larger, much-needed effort to examine the processes of decision-making in allocating research support.

DECISION

This recommendation calls for the design of a study which at least would be extremely difficult if for no other reasons than the definitional problems. Action should be deferred for more careful consideration and the development of the necessary methodology.

RECOMMENDATION #53 -- (#1 page 178 of the GPRST Report)

Recommendation #53 was considered with Recommendation #23 on page 8.

RECOMMENDATION #54 -- (#2 page 180 of the GPRST Report)

That, when, following completion of the initial technical merit review of a grant application but prior to final action by a National Advisory Council/Board, a BID complies with a principal investigator's request for a copy of the pertinent Summary Statement (as present NIH policy for implementing the Privacy Act requires), the BID should provide the document with priority score displayed and advise the requestor that the information being transmitted is interim in nature and that any attempt, at this point in the review process, to modify the original application or provide commentary for consideration by the Council/Board may result in a deferral of the application to the next review cycle.

DECISION

Because the priority score is only one element considered by the Advisory Council/Board, its release prior to final action could well be misleading. For this reason, recommendation 54 in its original form is not approved.

The following rewording is accepted:

"That, when, following completion of the initial technical merit review of a grant application but prior to final action by a National Advisory Council/Board, a BID complies with a principal investigator's request for a copy of the pertinent Summary Statement (as present NIH policy for implementing the Privacy Act requires), the BID should provide the document without priority score displayed and advise the requestor that the information being transmitted is interim in nature and that any attempt, at this point in the review process, to modify the original application or provide commentary for consideration by the Council/Board may result in a deferral of the application to the next review cycle."

RECOMMENDATION #55 -- (#3 page 181 of the GPRST Report)

That NIH should request authorization, through either regulation or legislation, as appropriate, to release an Initial Review Group Summary Statement to the principal investigator named in the application only after the review of his/her grant application is complete, i.e., after review by National Advisory Council/Board.

DECISION

Concur.

RECOMMENDATION #56 -- (#1 page 184 of the GPRST Report)

That a "single priority score" convention should be adopted for use throughout NIH.

DECISION

This recommendation, if implemented, would limit the options to be identified in the study described immediately below. Therefore action will be deferred on recommendation 56 pending completion of the study proposed in recommendation 57.

RECOMMENDATION #57 -- (#2 page 184 of the GPRST Report)

That, before adopting a single priority score notation system for use by all BIDs, the NIH should conduct a study of BID practices regarding the use of the "raw" and "normalized" priority scores, to determine whether the uniform NIH-wide convention should be the use of the raw score exclusively or the present normalized scores, whenever they are available (or otherwise the raw scores), or the development of a new procedure for computing, representing, and/or adjusting priority scores to compensate for differences in group rating behavior.

DECISION

Concur.

RECOMMENDATION #58 -- (#3 page 185 of the GPRST Report)

That, until such time as a single NIH-wide priority score notation system is adopted, all copies of Summary Statements which a given BID sends to principal investigators should display either the raw priority score or the normalized priority score (if available) but not both, depending upon which of the two conventions the particular BID follows.

DECISION

Concur. Whichever priority score (raw or normalized) is used by the Advisory Council/Board in making its decision on an application should be made available to the Principal Investigator.

RECOMMENDATION #59 -- (#4a page 185 of the GPRST Report)

NIH should conduct studies of variations in individual reviewer and review group behavior in rating applications, over time, and among different IRGs, and of the factors which act to increase or decrease such variability.

DECISION

Because of uncertainty as to the feasibility and adequacy of methodology for such a study, action will be deferred on this recommendation pending further discussion.

RECOMMENDATION #60 -- (#4b page 185 of the GPRST Report)

NIH should conduct studies of variations in the quality of grant applications assigned to a given IRG from one review round to the next, over time, and variations in the quality of grant applications assigned to different IRGs.

DECISION

Serious doubt exists as to the adequacy of means for measuring the quality of applications. Action will be deferred on this recommendation pending further discussion and study.

RECOMMENDATION #61 -- (#4c page 185 of the GPRST Report)

NIH should conduct studies of the effects on the review process of displaying the raw priority score to the Initial Review Group members immediately after they assign their individual scientific merit ratings, and of giving them the option, at that point, to reopen discussion and rerate the application.

DECISION

Concur.

The following eight recommendations were considered together:

RECOMMENDATION #62 -- (#1 page 193 of the GPRST Report)

That a formalized NIH grants peer review appeals system be established to correct or eliminate the deficiencies described above.

RECOMMENDATION #63 -- (#2 page 194 of the GPRST Report)

That, in addition to improvements in current processes for reconsideration of assignments or reviews, certain entirely new mechanisms be established for both types of appeal to handle situations which demand action or decision outside the original NIH components and at a higher level.

RECOMMENDATION #64 -- (#2a page 194 of the GPRST Report)

That, central to these new mechanisms be the establishment of the position of Ombudsman appointed by the Director, NIH.

RECOMMENDATION #65 -- (#2ba page 194 of the GPRST Report)

That, to provide the needed high levels of review related to appeals, there be established: a Grants Peer Review Appeals Board (GPRAB), a permanent committee, the members of which would act in matters concerning assignment of grant applications and those involving reconsideration of scientific review of applications.

RECOMMENDATION #66 -- (#2bb page 195 of the GPRST Report)

That, to provide the needed high levels of review related to appeals, there be established: Grants Peer Review Appeals Panels (GPRAP), temporary groups which would be formed to act in the final stages of disputes concerning the scientific review of grant applications.

RRECOMMENDATION #67 -- (#3 page 195 of the GPRST Report)

That the following criteria be established for reconsideration (appeal) of NIH action on grant applications:

RECOMMENDATION #68 -- (#4 page 196 of the GPRST Report)

That mechanisms and procedures be established for appeals concerning assignment of grant applications.

RECOMMENDATION #69 -- (#5 page 197 of the GPRST Report)

That mechanisms and procedures be established for appeals concerning scientific review of grant applications.

DECISION

This set of recommendations, all concerned with the establishment of a formalized grants appeals system, requires further discussion and study. We must determine the legal, fiscal, personnel and other implications of adopting the recommendations. Immediate consideration should be given to formalizing the present informal appeals system and making its function and operation widely known.

February 8, 1978

GRANTS PEER REVIEW

REPORT TO THE DIRECTOR, NIH

PHASE I

GRANTS PEER REVIEW STUDY TEAM

December 1976

MEMORANDUM

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE
NATIONAL INSTITUTES OF HEALTH

TO : Director, NIH

DATE: December 29, 1976

FROM : NIH Grants Peer Review Study Team

SUBJECT : Submission of Study Team Report

The NIH Grants Peer Review Study Team is pleased to submit to you the attached report. This report, Phase I, is based on a careful study of the issues by the Study Team and others at NIH and, in part, on the testimony presented at three public hearings, the letters received from the scientific community and the general public, and the results of a survey of NIH review group members. Phase II, to be completed in late 1977, will present a detailed analysis and evaluation of the letter responses and testimony from the public hearings; and will include the completed analysis and evaluation of the survey of IRGs and Council members.

The Study Team's report and recommendations are presented in Volume I. Volume II presents background appendices and Volume III is a collection of supplementary reports.

We have prepared a series of recommendations which we believe are central to the goal of not only maintaining but also improving the quality of the peer review system and thus of the biomedical research supported by NIH. We wish to call particular attention to several recommendations which represent a marked departure from the current peer review procedures or are deserving of special consideration:

- That a formalized NIH Grants Peer Review Appeals System be established, central to which is the establishment of a position of OMBUDSMAN to be appointed by the Director, NIH.
- That NIH should periodically announce all upcoming vacancies on initial review groups and invite suggestions regarding candidates for specific groups.
- That authority to establish or discontinue Initial Review Groups should be delegated to the Director, NIH.
- That NIH should seek to have the authority for selection and appointment of members of Advisory Councils/Boards delegated to the Assistant Secretary for Health, HEW.

- That those portions of the meetings of advisory groups which involve the review of grant applications should continue to be closed to the public (including those submitting applications), either under current exemptions to the open meeting requirement or through legislation.

- That, as soon as practical after a National Advisory Council or Board completes the review of a grant application, the Bureau, Institute, or Division should routinely send the associated summary statement with the priority score displayed to the principal investigator named in the application.

- That the Director, NIH, should take immediate steps to limit the workload of all Initial Grant Review Groups to a level compatible with maintaining the high quality of review.

The Study Team also addressed and developed recommendations concerning a variety of other key issues. These include identification and special consideration of unorthodox research approaches, the need for a single priority score convention for use throughout NIH in lieu of the present dual score system, conflict of interest procedures applicable to review group members, membership of employees of profit-making organizations in initial review groups, extension of peer review procedures to cover the assessment of business management practices in large grant applications, and opportunities for improving the grants peer review system through a continuing program of prospective studies involving specific procedures. Furthermore, the Study Team believes that all of the issues it considered are important and that, taken together, its recommendations form a self-consistent ensemble of actions which will do much to insure that the NIH grants peer review system remains a model of scientific and administrative excellence.

We believe that the report of the Study Team, and particularly its recommendations, will be of considerable interest to the biomedical community, the general public, and the NIH extramural staff. We have copies available for distribution if you wish to share the report with your staff or advisors.

Members of the Study Team are ready to assist you and your staff in the implementation of the recommendations presented in this report if you so wish.

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Chairperson

Robert P. Akers
Robert P. Akers

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PREFACE

The NIH Grants Peer Review Study Team is pleased to present this report to the Director, NIH, for his consideration. The April 28, 1975, statement of the Acting Director, NIH, established and set forth the charge to the Study Team. In one of a series of assigned responsibilities, the Study Team was asked to "Examine in critical detail the entire process of peer review and make, where necessary, recommendations for modifications or change...." It is hoped that the present study will be one of a number of other studies of the NIH peer review system, and that other study groups will address aspects of the peer review process not covered in this report.

Acknowledgements:

As Chairperson, I would like to acknowledge the efforts of all members of the Study Team and the Study Team subcommittees who took time from already busy work schedules to participate in this series of studies and deliberations. Listed hereafter are the Study Team members and the membership of the subcommittees.

Other individuals contributed to the work of the Study Team in special and important ways. Particular thanks go to Dr. Mathilde Solowey, who served as Executive Secretary to the Study Team. She played a crucial role in facilitating and coordinating the work of the Team, and joined as a full participant in our deliberations and the preparation of the report.

Ms. Pearl Cooper Williams, as consultant to the Survey Subcommittee, prepared the final survey questionnaire, supervised the distribution and collection of the survey questionnaires, and designed and conducted the analysis of the survey data and prepared lucid narrative comments, with the general guidance and support of the subcommittee. She has also prepared special analytic reports to relate the findings of the survey to important issues addressed by the Study Team. She was essential to the successful completion of the survey, and we are much in her debt.

Dr. Catherine Henley prepared the detailed description of the NIH peer review system as one of her first assignments at NIH, and participated in other Study Team efforts. We are most indebted to her for her contributions.

Mr. Sidney Gottlieb, Division of Management Policy, Office of Administration, made arrangements for the public hearings and the printing of this report and served as a staff resource. Particular acknowledgement is due for his excellent efforts.

Ms. Sue Fremeau, Committee Management Officer, NIH, was most helpful and served as an important resource.

Ms. Virginia Ono, secretary to the Chairperson, served as secretary to the Study Team with great distinction.

Particular thanks are extended to Ms. Alice Shoemaker for the major typing of the report. Others who have been of so much help in typing are Ms. Joan Clements, Ms. Sandra Shade, and Ms. Emily Johnson.

Ruth L. Kirschstein

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GRANTS PEER REVIEW

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VOLUME II

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GRANTS PEER REVIEW

REPORT

VOLUME III

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INTRODUCTION

INTRODUCTION

The past 20 years have seen a demonstrable growth of the extramural grants program supported by the National Institutes of Health (NIH). Specifically, support for NIH research grant awards grew from $32 million in FY 1955, to $473 million in FY 1965, to $1.12 billion in FY 1975. In 1975, 11,243 competing research grant applications were reviewed by peer review groups. Additionally, several thousand other kinds of applications were received, such as individual and institutional National Research Service Award applications, resource applications, and research career program awards. In 1975, the traditional investigator-initiated research grant applications requested a total of $887 million (for the first year only of multi-year applications). As a result of peer review evaluations, $431 million were recommended for approval and $318 million in first-year direct-cost funds were awarded for competing projects.

The level of interest in the NIH extramural research program and its operation is exceedingly high in the biomedical scientific community, which is increasingly dependent for research support on the availability of federal funds. Equally high interest is shown by the general public, which looks to this use of federal funds, for support of biomedical research, as a means of ultimately improving the health care of the nation.

The extramural research program is essential to the mission of NIH and it is therefore equally essential that it be of the highest quality. Since the inception of the extramural program at NIH, the gauge for selection of research and other grant activities for support has been by means of peer reveiw. Traditionally, NIH routinely monitors and administers the peer review system to assure its continued quality and effectiveness and to provide a continuous process of improvement.

Early in 1974, a two-day seminar was held by NIH officials to examine and appraise the extramural research and training grant review system, and the use of peer review and public advisory groups by NIH for its rapidly growing extramural research grants program.

A series of recommendations emerged from the seminar, including one specifically directed toward the need for an NIH Study Team to continue the study of the peer review system. Responding to the recommendation, in the spring of 1975, the Acting Director of NIH established the NIH Grants Peer Review Study Team (GPRST) consisting primarily of NIH personnel and including a member of the Office of General Counsel (OGC), DHEW, assigned to NIH. The Acting Director designated for membership on the Study Team, individuals whose expertise he considered desirable and necessary to accomplish the objectives.

Summary of the Charge to the Study Team

The Study Team was asked to:

> ". . . conduct a detailed and comprehensive study of the NIH peer review system. This study, in broad outline, should focus on the philosophy and procedures of peer review, its applicability to the

> NIH awarding instruments and programs, the attributes and problems of alternatives, and the role and character of peer review in the decision-making process at NIH."

More specifically, the Study Team was asked to:

> "Examine in critical detail, the entire process of peer review and make, where necessary, recommendations for modifications or change. In addition to other items which the Study Team may wish to consider, specific attention should be given to: (1) selection procedures and criteria for advisory committee members; (2) the need and feasibility for an NIH policy defining the administrative and scientific relationships of the staffs responsible for the technical merit review of grant applications conducted in awarding Institutes and Divisions and those responsible for scientific program management; (3) the need, advantages, and disadvantages for a technical merit review "appeal" mechanism for applicants and an assessment of the process currently in use; (4) the role and character of peer review in the decision-making process at NIH; (5) the capability of the peer review system to accommodate a really new or unusual scientific idea, . . . (6)assess the impact and make recommendations relative to peer review and provisions of the Freedom of Information Act (FOIA), the Federal Advisory Committee Act (FACA), and the Privacy Act (PA)." (Attachment 1 - Charge to the Study Team.)

The Study Team was instructed to examine the positive features of the peer review system as well as to assess critically any deficiencies that might be suggested: Overall, in examining the positive features, the Study Team feels that peer review exercises the single most powerful influence on the continued high quality of the Nation's biomedical research effort and encompasses several fundamental characteristics. These include the selection of participants who are judged according to their expertise and a system of checks and balances designed to protect the process from considerations extraneous to the criterion of high scientific quality.

Furthermore, the Study Team believes that confidence in the system is justified and has evolved primarily from the steadfast position maintained by NIH that it is, first and foremost, the high scientific quality of the supported research, as defined by the peer review system, which should govern the content of the national biomedical research effort. Allegiance to this principle by a succession of program managers at NIH, over a period of many years, has resulted in a research program not seriously challenged in regard to its fundamental, scientific quality.

The Study Team further believes that the NIH research programs constit critical national resource, and that the quality of this resource has t successfully defined in the public interest by the unique administrative device of the peer review system. It is confident that the system will continue to provide such service with careful management and maintenance and an unswerving commitment to the application of the principles of scientific excellence.

ORGANIZATION OF THE STUDY

The Team recognized that, although as a group consisting primarily of NIH officials, it could bring a significant collective perception of the peer review system as seen from the viewpoint of those who know the system best and who are also responsible for its operation, nevertheless, its appraisal of the peer review system would be strengthened by input from the external community. Accordingly, the Study Team sought opinions about the peer review system and suggestions for its improvement from the broadest possible range of biomedical researchers and interested lay individuals other than scientists.

To accomplish the tasks necessary to fulfill its mission, the Study Team was organized into a series of subcommittees. Each subcommittee was chaired by a member of the parent Study Team, but the membership generally included both Study Team members and other NIH staff as well. Participation of the latter served to expand the representation and provided added expertise. Progress and completed subcommittee reports were reviewed and modified as a result of discussion by the parent Study Team as a whole in order to assure concurrence and acceptance by all.

Certain specific issues were assigned to single Study Team members or small work groups in order to develop position papers. These served as a basis for discussion and for incorporation into the body of the report and as recommendations of the whole Study Team. These discussions generally resulted in one or more revisions prior to concurrence and/or acceptance of the document.

A brief statement regarding each of the subcommittees follows: A more detailed description of the subcommittees' activities and considerations will emerge from the body of the report.

- Subcommittee on Studies of Peer Review Systems

  The Subcommittee examined previous studies of the NIH Peer Review System and also reviewed briefly peer review systems in use in other federal agencies and other countries.

- Hearings Subcommittee

  The Subcommittee prepared for and planned three public hearings in different parts of the country to obtain the perceptions and/or suggestions of the external community in regard to NIH peer review. The Subcommittee also assessed, analyzed, and evaluated the information derived from the hearings.

- Survey Subcommittee

  The Subcommittee developed a survey questionnaire which was used to obtain opinions regarding the NIH peer review system from members of all current Initial Review Groups (IRGs) and Advisory Councils attending the November 1975-February 1976 meetings, and subsequently assessed, analyzed, and evaluated the findings.

- Subcommittee on Appeals

  The Subcommittee evaluated the desirability of establishing a grants peer review appeals mechanism, and, having determined the need for such a system, proceeded to develop appeals procedures.

- Subcommittee to Review Legal Aspects of Peer Review

  The Subcommittee examined the legal considerations related to peer review, such as Freedom of Information legislation, the Privacy Act, confidentiality, conflict of interest, chartering of committees, ad hoc reviewers, and selection of committee members, as well as the impact of these factors on the peer review system.

- Subcommittee on Business Management Practices

  The Subcommittee studied the impact of business management practices on peer review, and review procedures related thereto. It also reviewed and made recommendations concerning the establishment of policies regarding the role of a business consultant in peer review.

Additional topics considered by the Study Team and dealt with in depth in the body of the report or its appendices were the following:

1. Current procedures for selection of Initial Review Group, Special Initial Review Group, and Advisory Council members and modifications of these procedures.
2. The effect of review workload on quality of peer review, and suggestions to lessen the workload.
3. Summary statements and priority scores.
4. Premature disclosure of the outcome of peer review.
5. The role of peer review in support of innovative research.
6. The role of peer review in NIH decision-making.
7. The separation of peer review from program responsibilities.
8. The need for improved communication between NIH and the external community as well as within internal NIH components.
9. An analysis and evaluation of letter responses from the external community commenting on aspects of the NIH peer review system.

The Study Team also prepared a comprehensive description of the NIH grant application peer review process. (Appendix C).

ATTACHMENT 1

MEMORANDUM

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE
NATIONAL INSTITUTES OF HEALTH

TO : B/I/D Directors
ECEA Members
OD Staff

DATE: April 28, 1975

FROM : Acting Director, NIH

SUBJECT: Establishment of NIH Grants Peer Review Study Team

During the spring of 1974, the Executive Committee for Extramural Affairs sponsored and participated in a seminar on peer review. The purpose of this seminar was to identify and evaluate the objectives of the peer review concept, document strengths and weaknesses of the NIH peer review system, and discuss possible modifications or alternatives. This effort derived from the dual perception that NIH staff, who know the system best, should be in a unique position for its appraisal and that the system should indeed be subjected to periodic in-depth evaluation to assure its continued quality and effectiveness.

In addition to reviewing previous studies of peer review and the resulting suggestions and related criticisms, the ECEA Seminar format provided for a current appraisal of the Extramural Research and Training Grant Review System and a re-examination of basic concepts and procedures. This included a consideration of prepared papers followed by panel discussions and the ratification of a set of recommendations which were part of a final report of the seminar submitted to me in December, 1974.

Following up a major recommendation in the final report from the Planning Committee for the ECEA Seminar on peer review, I am establishing an NIH study team to extend the efforts of ECEA and conduct a detailed and comprehensive study of the NIH peer review system. This study, in broad outline, should focus on the philosophy and procedures of peer review, its applicability to the NIH awarding instruments and programs, the attributes and problems of alternatives, and the role and character of peer review in the decision-making process at NIH. In addition to this major charge, I am asking that the study team:

- Review previous and on-going studies and analyses of the NIH and other systems of peer review, evaluate recommendations and, in the course of the work of the study team, initiate additional analyses or activities as necessary for accomplishments of team objectives.
- Articulate in detail the philosophy, objectives, procedures, and accomplishments of peer review. From such descriptions, the study team should prepare or have prepared one or a series of papers for dissemination to the public and the scientific

community as a means of achieving a general understanding and appreciation of a system which has evolved to its present state over a period covering a quarter of a century.

- Examine in critical detail the entire process of peer review and make, where necessary, recommendations for modifications or change. In addition to other items which the study team may wish to consider, specific attention should be given to (1) selection procedures and criteria for advisory committee members, (2) the need and feasibility for an NIH policy defining the administrative and scientific relationships of the staffs responsible for the technical merit review of grant applications conducted in awarding Institutes and Divisions and those responsible for scientific program management, (3) the need, advantages, and disadvantages for a technical merit review "appeal" mechanism for applicants and an assessment of the process currently in use, (4) the role and character of peer review in the decision-making process at NIH, and (5) the capability of the peer review system to accommodate a really new or unusual scientific idea.

- Evaluate the need for enlarging or altering the type of NIH staff training and communications to improve and maintain coordination and interaction between separate NIH peer review activities.

- Assess the impact on and make recommendations relative to peer review and provisions of the Freedom of Information Act, the Federal Advisory Committee Act, and the Privacy Act.

To accomplish the above objectives and to consider other questions and recommendations concerning the peer review system for grants, the following study team is appointed by this memorandum:

Dr. Ruth Kirschstein	Director, NIGMS, Chairperson
Dr. Robert Akers	Policy and Procedures Officer, OERT, OD
Dr. George Brooks	Assoc. Director, Extramural Program Activities
Mr. Carl Fretts	Director, Division of Contracts and Grants, OA, OD
Dr. Norman D. Gary	Executive Secretary, SSS, DRG
Dr. William Goldwater	Assistant to the Associate Director, OCR, OD
Dr. Phillip Gorden	Acting Clinical Director, CI, NIAMDD
Dr. Jerome Green	Director for Division of Extramural Affairs, NHLI
Dr. Ann A. Kaufman	Research Grants Officer, OERT, OD
Dr. William Raub	Assoc. Director, Extramural & Collaborative Program, NEI
Dr. S. Stephen Schiaffino	Associate Director for Scientific Review, DRG
Dr. Katherine S. Wilson	Executive Secretary Genetics SS, DRG

In pursuing its work, the study team is requested to make full use of all necessary resources both within and external to the NIH. Within this framework, it is expected that appropriate consultation will be made with members of the scientific and grantee community and current and former members of NIH Advisory Groups in order to examine perspectives, criticisms, and suggestions for alternatives or improvements to the peer review system. The office of the Associate Director for Extramural Research and Training will make available to the study team the full report of the ECEA Seminar on peer review and other such documents and previous studies that will be useful in its deliberations.

I am asking that the study team report interim activities to me through the Associate Director for Extramural Research and Training and that a final report with recommendations be submitted no later than May 1, 1976.

I would greatly appreciate all cooperation and assistance that you and your staff can render the study team. In addition, if there are comments or suggestions which you or your staff feel may be useful to the study team, please communicate them directly to Dr. Ruth Kirschstein.

RW Lamont-Havers
Ronald W. Lamont-Havers, M.D.

STUDIES OF PEER REVIEW SYSTEMS

11

STUDIES OF PEER REVIEW SYSTEMS

Apparently, peer review procedures for health research supported by the Federal Government were initiated in 1902. At that time the Fifty-seventh Congress established a Scientific Advisory Board of non-Governmental scientists to assist the Surgeon General in the administration of the Hygienic Laboratory. In later years (1930) the Laboratory was renamed the "National Institute of Health" and the Advisory Committee was reconstituted and renamed the "National Advisory Health Council."

When the National Cancer Institute was established in 1937, the Cancer Act also provided the legal basis for the National Advisory Cancer Council. The Council was to play a key role in recommending the award of grants. These authorities and procedures were extended to grants and fellowships in all health research areas in 1944, when the Public Health Service Act was passed.

In 1946 the Director of the NIH recognized the need to establish a more formal advisory mechanism for the assessment of technical and scientific merit; the Division of Research Grants (DRG) was established and study sections or initial review groups came into existence in 1947. Thus, at that early date, the basis for the current NIH dual review system was established.

Over the years, since the very inception of the dual peer review system, there have been multiple studies of the NIH advisory structure and the review and approval system. As NIH review procedures have matured and developed, and as the extramural programs have expanded, these studies and surveys have become more frequent and extensive. Some were instituted by the Executive Office of the President, some by Congressional mandate, and some by the Department of Health, Education, and Welfare (DHEW) or the staff of NIH. The members of the Grants Peer Review Study Team (GPRST) have cataloged and reviewed these previous studies. Some focused in particular on NIH while others were of a more general character dealing with overall Federal policies. Those studies that seem particularly relevant to the current undertaking are briefly reviewed here.

In 1947, the same year that the DRG was established, the President established a scientific research board to study and report on Science and Public Policy. Volume 5 of its report is entitled "The Nation's Medical Research."(1) Even at this early stage, the Board noted that the use of advice from outside experts on research proposals presented a difficult dilemma. While the selection of experts from the leading hospitals, medical schools, and research institutes insured the best possible advice, on the other hand, as the Board noted, the majority of proposals to be evaluated were submitted by staff members or graduates of these same institutions. It was concluded that the advisory groups were providing splendid service to the Government and devoting conscientious thought and considerable time to the review and evaluation of the proposed projects while resisting any tendency to create research monopolies. The Federal Government was advised to continue its sound policy of utilizing outside scientific advisers.

In the mid-1950s, the Secretary of DHEW commissioned a report on "The Advancement of Medical Research and Education Through the DHEW." This report, commonly known as the "Baynes-Jones Report," was issued in 1958. (2) The consultants recommended the expansion of medical research and education, and projected that total national medical research expenditures should increase from the 1957 level of $330 million to approximately $1 billion per year by 1970. The report briefly described the NIH peer review system and noted that the advisory structure and procedures were designed for a relatively small grants program characterized by grants of modest size. The report predicted that the NIH extramural program of research support would evolve towards larger grants for broadly defined purposes encompassing many disciplines and would require changes in the traditional study section system.

In 1960, the Senate Committee on Appropriations requested advice from a committee of consultants on Federal support of medical research. There is no indication (in this so-called "Bo Jones Report") that the appointed committee made an indepth study of review procedures. However, in determining that funds appropriated by the Congress for the support of research on major disease problems had been expended by Federal agencies with remarkable efficiency, the Committee report included the following statement:

> "The National Institutes of Health, which Congress has made its principal instrument for the disbursement of funds for medical research, has developed an extremely successful system for review and approval of applications for research grants, training grants, and fellowships. This plan, relying on close supervision of outstanding scientists, has assured consistently high standards for the research supported, gained the confidence of the scientific community, and maintained the traditional freedom of both institutions and investigators." (3)

Many similar statements were made in the November 1960 Report of the President's Science Advisory Committee (PSAC). "In its support of basic research, the government has usually relied on the advisory judgment of respected scientists, and in the main this advice has ensured that in those areas of research in which Federal support has been available, outstanding men have been able to attract substantial support. In this respect, the project method of research support has real values which should not be forgotten...." (4)

The Committee on Science and Public Policy of the National Academy of Sciences (under the Chairmanship of Dr. George B. Kistiakowsky) issued a report in 1964 concerning "Federal Support of Basic Research in Institutions of Higher Learning." (5) This report gives a history of the origin and the development of the Federal scientific establishment and discusses the use of grants and contracts, and peer review procedures, in a variety of Federal agencies. The study had its origin in a resolution passed by the American Society of Biological Chemists which requested an examination of Federal programs in support of fundamental research. The Committee expressed support for the panel system of peer review. However, disadvantages of the panel system of peer review were mentioned, including the workload accepted by scientists who participated, the increased time to complete the review of each proposal, and the potential difficulties when service on peer review groups is concentrated among too few individuals or institutions. The Committee also noted potential problems with interdisciplinary research, namely, that panels may not always reflect the current frontiers of research, or that the proposals may be sent to the wrong panels. Recommendations for the improvement of panel peer review were provided: (1) the use of more advisors and more frequent rotation; (2) the use of younger scientists on panels; (3) the inclusion of persons of breadth as well as specialization; the addition of members from allied fields; (4) the serious consideration of the advice of panels in order to maintain the conviction among advisors that their services are important; (5) the fact that overall budget review is appropriate to peer review, but detailed decisions on fiscal matters are not; (6) continuation of support of basic research by several agencies so that scientists will have access to more than one peer review process.

In that same year, 1964, the Select Committee on Government Research of the House of Representatives (the Elliott Committee) issued its report on national goals and policies. This Committee was established to provide the House of Representatives with an overall review of Federal research and development programs. Although none of the ten studies reported dealt primarily with NIH or with peer review, the Committee examined various methods used by Federal agencies to review applications for support. The Committee concluded that fellow scientists must rate both the competency of those presenting proposals and the ripeness of the field but that the evaluation criteria used must include value, economic, and political judgments, as well as broader judgments concerning scientific and technical matters. The Committee noted that the panel systems utilized by various agencies provided a training ground for the development of a pool of advisors required at higher policy levels. (6)

Perhaps the largest, most extensive, and most prestigious study of NIH was the one commissioned by President Kennedy and chaired by Dr. Dean Wooldridge. The Wooldridge Committee, supported by twelve working panels, submitted its report on "Biomedical Sciences and Its Administration" in 1965. (7) Detailed descriptions and analyses of the NIH peer review procedures are contained in various sections of that report, but particularly in the report of the Panel on Review Procedures. The first and most important general conclusion of the study was that the activities of NIH were essentially sound and that funds were being spent wisely and well in the public interest.

> "The opinion of the (Wooldridge) Committee, based on the extensive investigations of its consultants, is that the large majority of the intramural and extramural research supported by NIH is of high quality. We strongly approve the peer evaluation method of selecting recipients of extramural grants."

> "The Study Section Procedure utilizing scientific peer judgments is the best available method for awarding research grants." (7)

It is believed that the Wooldridge Committee conducted the most comprehensive and thorough review ever undertaken of a Federal science agency. Addressing itself specifically to the extramural programs, the Committee felt that despite increased loads on study sections, peer judgments should be preserved at all costs. Measures should be encouraged which would lessen the burden on study sections without usurping their function of scientific review. The Committee felt that extramural investigators would like more contact with and advice from scientists on study sections and would welcome more site visits, suggestions from study sections about improving research plans, and more explanations of why proposals were not approved.

In 1966, no fewer than three important reports were published. The Westrate Report, commissioned by the Bureau of the Budget, dealt with the management of Federally funded research programs in several areas, including biomedical sciences. Information was obtained by interview and questionnaire methods that involved Federal officials, university staff members, and officials at private foundations. The section of the report most relevant to review procedures is entitled "Improving the Quality of Federal Research Administration" and portions of this deal specifically with NIH methods, viz., "the most elaborate review procedures of any agency." (8)

The report recommends that:

> "Appropriate action should be taken to remove the statutory provision which prohibits the Surgeon General from authorizing approval of an NIH grant unless the grant has been expressly approved by the Advisory Council of the pertinent Institute.
>
> "With respect to proposal review procedures, the agencies should:
>
> a. continually evaluate the adequacy of the advice they obtain from proposal reviewers and the procedures they employ;
>
> b. provide sufficient administrative assistance and numbers of advisors to reduce the reviewers' workloads and insure that their time can be most profitably utilized;
>
> c. because of the advantages inherent in panels, use panels for scientific review to the maximum extent feasible; and
>
> d. when panels are used, rotate the membership and give considеation to geographic and university distribution in selecting appointees.

"In addition to their present procedures for dealing with possibilities of conflict of interest, agencies should:

a. stress the advisory nature of panel deliberations, and instruct the Government member present at such panel sessions to deal with potential conflicts which may arise.

b. bar a consultant or panel member from participating in general discussion about proposals submitted from any source in his own university, although he should be permitted to answer specific questions of fact regarding such proposals.

"As part of their review procedures, all agencies should request panel or consultant opinions regarding the general level of funding, major equipment requested, and fraction of investigator effort estimated for the project when such opinions appear to be relevant to deciding the merits of the proposal.

"Agencies should routinely distribute an award list to their proposal referees." (8)

The report of the Secretary's Advisory Committee on the Management of NIH Research Contracts and Grants (the Ruina Committee) was also published in March 1966.(9) Once again, the review procedures utilized in NIH extramural programs were examined and endorsed. The report of the Ruina Committee stated that:

> "With the enormous expansion of the grant activity during the past decade, the Advisory Councils have found it necessary, as a practical matter, to rely almost entirely upon Study Sections, panels of distinguished scientists organized on the basis of scientific disciplines and medical specialties, to evaluate the scientific merits of grant applications. So large a volume of applications is now processed that Advisory Councils are almost entirely dependent upon summaries and "priority ratings" forwarded by the Study Sections.
>
> "This inevitable dependence on 'peer groups' for preliminary screening and ordering of grant applications has not resulted in a mere 'rubber stamp' function for a Council. Rather, policy deliberations undertaken by a Council are often based upon questions concerning individual grants which are singled out by notations of the Study Sections, or by NIH staff or members of the Council at the time of meeting. Each Advisory Council member receives summaries of all of the applications for grants, and thus has a convenient reference file of current information to assist him in keeping up with the broad trends in research pertinent to his Institute's field of interest.

> "The effectiveness of these project grants in support of biomedical research is widely recognized, as is exemplified by the following comment of one of the panels of the Wooldridge Committee.
>
> "'The procedures followed in reviewing what are known as traditional research grants were instituted first and constitute the most thoroughly established, the most rigorously followed, the easiest to understand, probably the most widely admired of the activities included in the extramural programs of the National Institutes of Health.'"(8)

Later, in 1966, a special Congressonal subcommittee chaired by Congressman Paul G. Rogers investigated DHEW.(10) With respect to the function and adequacy of the NIH Advisory Councils, the Subcommittee endorsed the critique of the Ruina Committee. The Subcommittee noted that the NIH system of review and approval was considered desirable by the scientific community primarily because proposals were independently judged by peers and not by Government officials. The NIH grant review system thus separated the assessment of scientific merit from the staff responsibility for program administration. In the opinion of the Subcommittee, one of the weaknesses of the NIH system resulted from the fact that it was devised for a much smaller volume of grant applications; it was noted that it was virtually impossible for review groups, including the Councils, to conduct completely satisfactory review in the limited time available.

Among other things the Rogers Subcommittee stated that:

> The increase in the number of study sections had not kept pace with the increase in the number of applications; the explosion of scientific knowledge as a result of increased emphasis placed on research; the unrealistic assumption that the combined knowledge of the members of study sections and councils will in each case include all the latest information available in a particular specialty; consequently in view of the latter, recommendations for approval or disapproval appear generally to be the judgment of one or at best two peers. It was recommended that consideration should be given to revising the system for review and approval of grant applications in order to better cope with the large volume and high degree of complexity of the applications. This report also pointed up the fact that certain questions raised by the Wooldridge Committee still remained unanswered; namely, does the study section discussion modify a priority rating based only on reading the application prior to discussion; what determines which applications are site visited; does the site visit modify an original priority rating—in which direction; how is the quality of study section decision evaluated? (10)

In 1967, the American Medical Association's Commission of Research delivered its report.(11) Many of the recommendations of this Commission were addressed specifically to the American Medical Association. The Commission did conclude that the study section mechanism of NIH had been successful in identifying and supporting high quality research. The Commission recommended that membership on study sections should be rotated to prevent the development of an inbred, elite, decision-making establishment. The report also suggested that the primary consideration in selecting members of Advisory Councils should be professional attainment and the ability to make unprejudiced, statesmanlike judgments. The Commission also recommended the establishment of an advisory group for the Director, NIH.

The Fountain Committee Report in October 1967 (12) examined "The Administration of Research Grants in the Public Health Service." The report focused upon the principal research arm of the PHS, the NIH. (It should be recalled that the Committee had issued previous reports on this subject in 1961 and 1962.) The Committee was concerned about the gap in NIH research support between the "rich" and "poor" schools. One reason given was that a relatively limited number of institutions furnished the bulk of the PHS consultants and it was felt likely that these advisors reacted more favorably to the institutions and scientists that they knew best when evaluating project applications. Moreover, study section and council members were in a unique position to learn of research opportunities in their fields and to share with their colleagues an intimate knowledge of how the grant system operated. A second concern was the tendency, in PHS, to appoint a small group of individuals to multiple terms on councils and committees and the appearance of favoritism when these same individuals received substantial NIH grants. The Committee recommended that each council appointment be limited to one four-year term with members being ineligible for reappointment for a period of four years. It also recommended that consideration be given, in the selection of committee members, to balanced representation of geographic regions and educational institutions and, further, that to the extent possible, consultants should be drawn from among qualified scientists who are not themselves recipients of PHS grants.

In February 1968, the DHEW issued a response to the Fountain Committee Report.(13) There are, in particular, two sections of this 98-page report that are especially relevant to the interests of the current NIH Grants Peer Review Study Team. The first discussed the review system at NIH and its role in maintaining the high quality of research that was being supported. In the section on advisory committees, recommendations were made in regard to limiting the period of appointment of NIH advisors, geographical distribution, and to the development of standards to insure that scientists of high quality are appointed to advisory groups.

In 1970, the Council of Academic Societies of the Association of American Medical Colleges (AAMC) undertook to formulate "A Policy for Biomedical Research."(14) The Committee focused particularly on issues relating to the development of biomedical research manpower. The report supported continued emphasis on the individual project grant awarded through peer review as the primary instrument for the support of biomedical research, along with an expanded system of program project support addressed to problems of special relevance. The report included strong endorsement of peer review but stated that review mechanisms should be streamlined. There were, however, no detailed discussions or specific recommendations.

During the past several years, there have been increasing inquiries into the NIH peer review system. Some of these have followed upon the passage of the Freedom of Information Act (P.L. 89-487) and the Federal Advisory Committee Act (P.L. 92-463), and have stimulated a general review of advisory group activities and functions within the Federal Government. Within DHEW, additional controls and requirements have now been established with respect to the public advisory groups and these apply to the NIH public advisory groups. Thus, procedures for the technical merit review of scientific research proposals have been modified.

Among the studies and surveys that have led to such modifications, one must include that of the "Cooper Committee."(15) The report of this NIH Program Mechanisms Committee was issued in February 1973 to the Director, NIH. In this report, the Committee included seven recommendations bearing upon the grant and contract programs. Especially noteworthy is the recommendation "that NIH establish and refine the use of uniform policies and standard procedures by which its components define, develop, and implement programs, and initiate, review, select, and manage projects funded by contracts, grants, and other awards."(15)

In April 1973, NIH and the Health Services and Mental Health Administration (HSMHA) received an issue paper prepared by the Office of Management and Budget entitled "The NIH/NIMH Peer Review System."(16) The paper contained a generally accurate, brief description of NIH/NIMH peer review, including its statutory bases. In its identification of problems with the peer review system and its formulation of alternatives for improving the system, however, the paper displayed several serious misunderstandings and biases. Nonetheless, the report, and the commentary prepared by the Office of the Director, NIH, are of considerable interest. Both documents were considered and discussed in the July 1975 special oversight hearings of National Science Foundation (NSF) peer review (House Subcommittee on Science, Research, and Technology).

The requirement in the National Cancer Act Amendments of 1974 (17) for scientific peer review of applications for biomedical and behavioral research apparently resulted from the hearings before the Subcommittee, particularly the prepared statement from the AAMC. In urging peer review for contracts, the AAMC stated that (1) the use of nonfederal scientists to review grant applications for biomedical research projects to be funded by the Federal Government has assured a broad, nonfederal voice in the formation and implementation of national policy; (2) the peer review system which has been developed and utilized for NIH grants has assured a rigorous assessment of the scientific merits of research projects for which NIH grant support is being sought; (3) this process of scientific appraisal carried out by disinterested and expert scientists has resulted in the use of public funds only for the support of biomedical research which has met the highest standards of excellence; and (4) the process of peer review has been recognized as an effective mechanism in assuring maximum scientific returns for public investments in research grants.

In one of the few studies dealing with the effectiveness of decisionmaking in providing research support, Grace Carter of the Rand Corporation developed an original and rather ingenious, statistical method to examine NIH management and peer review decisions. Measures of research output and quality were developed and these were then used to explore trends in the quality of grant-supported research and the operation of the NIH peer review system. This 1975 report, entitled "Peer Review, Citations, and Biomedical Research Policy: NIH Grants to Medical School Faculty,"(18) is of considerable interest and value.

It is noteworthy that the NIH, in March 1975, submitted a report (19) to the then existent President's Biomedical Research Panel concerning the status of implementation of the recommendations of the Wooldridge Committee. The reader should recall that, in February 1965, the Wooldridge Committee (7), established by the President, made eighteen recommendations. Eleven of these were accepted in whole or in part and implementation was begun; seven recommendations were not accepted. Among the recommendations that related directly to peer review, there was a statement "that Advisory Council members owe their appointment to no higher Government level than that of the Director, NIH." The NIH noted that this recommendation had not yet been implemented. The Wooldridge Committee also recommended "that the study section procedure utilizing peer judgments should be preserved and strengthened and that the workload on individual study section members should be lessened."(7) The NIH status report indicated that this had been implemented initially but was affected by later changes. The workload of consultant reviewers has been seriously aggravated and the number of committees has been sharply curtailed. It is noteworthy that the average workload of an NIH Study Section has increased remarkably: from an average of 55 grant applications per meeting in 1969 to an average of over 90 applications per meeting in 1976.

OTHER REVIEW SYSTEMS IN THE FEDERAL GOVERNMENT

The NIH Grants Peer Review Study Team also undertook an examination of the review procedures in several other Federal agencies; this was conducted in the fall of 1975. In two agencies the review procedures appear to be especially relevant to the NIH. A brief summary of review procedures and policies in several other Federal agencies is given below.

I. THE NATIONAL SCIENCE FOUNDATION (NSF)

There are five separate directorates at the NSF and there are, as a consequence, five separate review systems. These are described in the June 1975 NSF Staff Study on Peer Review and Proposal Evaluation. In general, NSF utilizes three distinct options: panel reviews, mail reviews, and staff reviews. On the average, for all of NSF, a proposal is reviewed by 6.5 reviewers. There is no single list of NSF reviewers but lists of panel members are available. Forty-four percent of all competing proposals receive an *ad hoc* review only. Twenty-eight percent of all proposals are reviewed by a panel only. Some 28 percent of proposals receive both panel and *ad hoc* review. Some proposals submitted to NSF are reviewed solely by staff. These, however, are a distinct minority and represent such activities as conferences and workshops.

During 1974, the NSF received approximately 120,000 reviews. (In contrast, NIH reviews about 12,000 applications.)

In each of the NSF directorates, it is the program officer who is responsible for formulating a recommendation. The selection and the funding decision are made at a higher echelon. The program officer makes his recommendation to a section head but often this is referred on to a division head, assistant director, or even to the National Science Board. The Board reviews those proposals that are favorably recommended by a panel and that involve more than half a million dollars per year or a total commitment in excess of two million dollars. The Board does not review proposals recommended unfavorably.

Recently, the National Science Board passed a resolution indicating that full lists of reviewers should be published and that verbatim copies of reviews, without the reviewers' signatures, should be forwarded to the applicant investigator. NSF is currently informing reviewers that their verbatim comments will be made available to applicants.

Panels rate proposals that they have recommended favorably in five categories from excellent to poor and these roughly correspond to the NIH numerical rating system of 1 through 5. Staff provides feedback and reports to the panel at subsequent meetings concerning actions that have been taken on their panel recommendations. Although NSF staff members have not done so in the past, they will now prepare summary statements and provide resumes of the panel assessments and recommendations so that this information may be released upon appropriate request. A formal procedure for appeals is being developed at NSF but it is not expected that it will be used extensively.

Within the Biology Division, some 4,000 proposals are reviewed each year; approximately one-third are renewal applications. About 30 percent of all competing proposals are funded. Currently, some 35 percent of the funds available are used to award committed, non-competing continuation grants. Within NSF, it has been decided (administratively) that non-competing continuations should not utilize more than 60 percent of the available funds; this guideline has been set in an attempt to maintain flexibility for the support of new ventures. The maximum duration of grant support in the past has been limited to two years but it has now been extended to five. Therefore, the average duration of NSF grants is gradually increasing.

A visiting committee evaluates each NSF program area and the performance of the program officer, and provides advice to the Division Director and to the Assistant Director of the Foundation. It is of some interest to note that about 50 percent of the program directors at NSF are there on a rotational system while on leave from their parent institutions. These people are selected by the Division Director or the Assistant Director, NSF. The NSF staff believes that this system has worked well. Of the 24 scientists currently in the Biology Division, 12 are there on a rotational basis.

Within the Biology Division, the average research grant amounts to some $34,000 in total direct costs. There are, however, approximately 40 larger grants, somewhat similar to NIH program project grants, that range in size from $250,000-$500,000 per year; most are near the upper end of that range. The total expenditures for the Biology Division in support of research are 75 million dollars per year.

The average panel used by the Biology Division has eight members and there are 12 panels. The panels know in advance the amount of funds available for each program area and this information is taken into account in the proposal review sessions. Proposals that are reviewed are either funded or declined. Thus, there are no approved proposals that remain unfunded. This is a feasible and effective procedure for the NSF because its appropriation is obtained early in the fiscal year and it is therefore able to plan expenditures in advance.

The review panels meet three times a year, generally for two days. The average number of proposals reviewed per meeting is 100. Each proposal is, on the average, assigned to four reviewers. Site visits are conducted but not frequently. Travel funds are somewhat limited; oftentimes a site visit will be performed by staff members only.

Consultants are paid a fee of $75 per day but do not receive any remuneration for homework or mail reviews.

NSF panel members are appointed for a one-year period which may be renewed two times. Thus, most consultants serve a three-year term. In the Biology Division, at the beginning of each panel meeting a listing of the proposals and the ratings given by each of the panel members is distributed. Thus, with this information available, the greater part of the panel meeting focuses on discrepancies in ratings. The Program Director serves as chairman of the panel.

There are no formal deadlines for the submission of applications to NSF. In the Biology Division there is an average lapse of four months from the receipt of a proposal to the time that a decision is made and funding is made available.

The Division has a small contract program and issues "Requests for Proposals" in the usual manner. Contract proposals are reviewed by ad hoc committees.

There are four changes that the NSF staff would like to implement:

(1) to increase the number of panels for initial review;
(2) to decrease the panel workload to no more than 60 proposals per meeting;
(3) to perform more project site visits; and
(4) to have more staff available.

The NSF is planning to conduct several studies of its review processes. For example, the handling and fate of 1,000 applications will be studied. In addition, a survey (questionnaires and interviews) of several hundred applicants and reviewers will be conducted to determine their views of the NSF review system.

II. THE VETERANS ADMINISTRATION (VA)

The VA system for peer review of medical research is essentially an intramural system. There are some 135 stations in the VA system where research is conducted. Thus, this is basically a decentralized intramural medical research program. Seventy-five percent of the budget is devoted to that decentralized system whereas the remainder is centralized and involves cooperative clinical trials and career development awards. Centralized peer review is required if more than $25,000 per year is expended in a research project.

Initial review is conducted centrally by merit review boards and the secondary level of review is conducted by VA administrators. The membership and nature of the second level of review have been developed to reflect the intramural nature of the research effort: difficulties with staff and employees in field stations are avoided; orderly termination and phaseout support can be provided readily; and morale problems can be minimized or resolved.

Last year a total of $87.7 million per year was spent for medical research and $68 million of that amount was for decentralized research studies.

An attempt is made to fund all approved proposals to some extent. Furthermore, to preserve local allegiances and authority, funds are forwarded to the hospital for the support of the investigator's research but these funds can be reallocated by the local research committee.

The procedures for peer review have varied somewhat with time. Initially, boards were established and review was conducted centrally. There was then a brief period when there was total decentralization of the review and decision-making processes. For the past three and one-half years the current system has been in operation.

Currently, there are 14 merit review boards and each has 7-13 members. The boards are disciplined-based and they meet twice a year for one to two days. Members have three-year terms. Each board has at least one VA member, but overall, 25 percent of the total membership are VA employees. Many of the individuals serving on these merit review boards are, concurrently, NIH study section members. There are two cycles per year for review with December 1 and June 1 deadlines.

The 14 merit review boards are serviced by six staff members: three executive secretaries and three assistants. It is apparent, therefore, that one of the executive secretaries and one assistant serve five merit review boards.

A proposal may be reviewed by more than one review board at the same time. Twenty-five to 30 proposals are reviewed at each meeting. A numerical priority system is utilized with a score of 10 being the best and 50 the poorest. The average score is 27; any score numerically greater than 40 indicates that funding is most uncertain. Priority scores are derived by averaging individual scores voted by the members; they are not "normalized."

Each proposal is reviewed by at least three reviewers, one of whom must be a member of the board. Thus, many outside opinions are obtained. Each ad hoc reviewer is contracted by VA staff before the proposal is sent to him. It is of interest to note that the evaluations and opinions submitted by the reviewers are transmitted verbatim (but without the signature of the reviewer) to the hospital that submitted the proposal and thus to the applicant institution and the investigator. Each applicant receives at least three written evaluations prepared by individual reviewers along with the overall evaluation of the review board.

Approximately 35 percent of proposals are disapproved. The summary statements of the merit review board are written by a board member. Site visits by the board are not conducted frequently but if they are held they are generally small with a member of the VA staff of the central office always in attendance.

Recommendations made by the review boards are similar to those made by NIH Study Sections in terms of dollar amounts, duration of support, etc.

The review board meetings are closed although announcements are made in the Federal Register. Any public or scientific inquiries received are referred to the local VA hospital.

There is no formal appeals mechanism. It must be recalled that this entire review system is concerned with intramural research activities. Thus, a disappointed applicant must have the agreement of his local hospital research committee before he can submit a new or revised request to the central office. The local research committee often has many academic scientists from the medical school/university. For that reason local review is a significant phase of the activity.

Reviewers assisting by mail in this process are paid $50 for each opinion. Members of the merit review boards receive one day's compensation for their preparation time.

It should be noted that the peer review system described above is not universal throughout the VA. Cooperative clinical studies are assessed by an entirely separate procedure. They are reviewed and budgeted through a different mechanism. Similarly, the career development program of the VA is reviewed and funded through a separate set of procedures.

III. ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION (ERDA), DIVISION OF BIOMEDICAL AND ENVIRONMENTAL RESEARCH (DBER)

The major portion of the Division's support, about 80 percent, is for the National Laboratories; the remaining 20 percent is for the support of extramural projects. In 1975, this constituted approximately 20 million dollars.

ERDA supports solicited contract proposals submitted in response to a request for proposal (RFP) as well as unsolicited proposals. The latter are similar to NIH research grant proposals and procedures for their review will be described.

Unsolicited proposals may be submitted at any time during the year. Upon receipt, review is begun immediately. If handled normally, the review can be completed in three months or more rapidly, if necessary. Unsolicited proposals are referred to the appropriate Branch (currently there are five Branches), reviewed by the Branch Chief, and assigned to the technical staff representative whose scientific and technical expertise best matches the research proposal.

DBER uses a mail review procedure. A list of reviewers and their specialty areas is maintained and a general effort is made not to use the same reviewer too frequently. The technical representative sends the proposal to 3-6 outside reviewers and receives written reports from each. The consultants review for scientific merit only whereas the staff representative reviews for relevance to the mission and for budget considerations. The staff representative has the authority to reject the recommendations of reviewers; but if he does so, he must defend his action.

The staff representative's summary and the proposal are circulated to relevant technical staff within the Division for input from other experts, usually so-called "category teams" from each of several Branches of the Division who review the proposal. The proposal and summary report are then reviewed by the Research Committee of the Division, which is composed of the technical staff of the Division. This Committee meets weekly and the responsible staff representative presents a summary, recommendation, and the reviewers' opinions of each application. The Research Committee votes approval or disapproval and approved proposals are ranked for payment.

There is one budget for the Division. This allows for flexibility to shift funds where needed to support approved projects. If necessary, approved projects can be held until funds are available.

In 1975, the Division supported about 500 contracts and there are about 400 renewals yearly. Ongoing contracts are reviewed by outside consultants every third, sixth, and ninth year and the reviews are documented. New proposals over a specified dollar amount must be "site visited," usually by the staff representative, who must submit a written report prior to the initiation of the regular review.

IV. NATIONAL AERONAUTICAL AND SPACE AGENCY (NASA), OFFICE OF LIFE SCIENCES (OLS)

The NASA program is undergoing restructuring from a mission-oriented program to a problem-oriented program. Decentralization of funds and problem clusters are to be allocated to the three field laboratories. The procedure prior to the planned restructuring was to have all proposals received in the central office, then sent to the relevant field laboratory for prescreening prior to initiation of peer review which was conducted by the central office. A brief discussion of general review procedures will follow.

NASA carries out its mission via contracts and grants: contracts with industry, grants with universities and non-profit institutions. The nature of the project determines the choice of instrument. The review of unsolicited grant proposals will be described briefly as these proposals are similar to NIH investigator-initiated grant proposals.

Proposals are reviewed for scientific merit by panels convened quarterly. A contract with the American Institute of Biological Sciences (AIBS) provides for the management of panels, selection of members, conduct of review, and preparation of written reports evaluating the proposals. Membership on panels is on an ad hoc basis. Selection of reviewers is made as required to provide the expertise for the proposals to be reviewed at the specific quarterly meeting. There are deadlines for receipt of applications to meet the quarterly panel reviews. Although AIBS selects the panel members, there is input to the selection process form OLS. The panels review for scientific merit only, recommending approval or disapproval. If approval is recommended, the proposal is ranked for quality from 1 to 4. Intermediate rankings for quality often require additional information and/or negotiations with the applicant. OLS staff reviews proposals for program relevance. OLS staff prepares summaries and recommendations based on the two reviews. Funding decisions, made by OLS staff, are based on the scientific recommendations and mission relevance.

In 1975, it was estimated that approximately 3.5 million dollars was available for research grant support in biomedical research by this agency.

V. OFFICE OF NAVAL RESEARCH (ONR)

ONR supports mission-oriented basic research in four program areas. Proposals are reviewed by outside consultants convened on an *ad hoc* basis. ONR contracts with AIBS to select consultants and to conduct the panel reviews. ONR has no statutory requirement to provide peer review by outside consultants, but finds such reviews acceptable, and is generally guided by the recommendations. ONR may and frequently does recommend consultants to AIBS for the panels. The consultants review for scientific merit only. Proposals are rated on a rating sheet designed by the panel. With the information provided by the reviewers, ONR staff decides what should be supported, ranking proposals on the basis of relevance to the mission of the Navy.

Most contracts are for 4-5 years and there is about a 20-25% turnover. In 1975, ONR supported about 300 contracts including renewals, totaling approximately 15 million dollars. ONR does not anticipate an increase in funding or expansion of this program.

VI. UNITED STATES DEPARTMENT OF AGRICULTURE (USDA); COOPERATIVE STATE RESEARCH SERVICE (CSRS)

CSRS awards grants and a small number of cooperative agreements. Additionally, it has two formula grant programs and a discretionary special grant program. The special grant program is most comparable to the NIH extramural programs. In 1975, CSRS funded about 150-160 projects for a total of about 15 million dollars. CSRS anticipates about the same level for 1976 but may have a slightly higher budget approximating $19.5 million.

CSRS staff obtain outside expert opinions from "evaluators" for each proposal. Selection of experts is made by staff. The number of evaluators may vary according to the proposals. A standard list of ten technical and two "impact" criteria is used for evaluating proposals and a 0-10 ranking system is used. Conflicting recommendations of evaluators are resolved by further discussions with CSRS staff. Additional information may be solicited from the applicant, and additional evaluative opinions may be sought. Projects to be supported must fit program allocations as identified by the Department or by Congress.

Budget allocations in various program areas, criteria for review, and format for grant proposals are published in the *Federal Register*. Proposals are received within one month after announcement in the *Federal Register* and the review process proceeds immediately thereafter.

Rejected applicants are usually provided with a summary of the critical elements which may have accounted for the rejection.

The program of the Agricultural Research Service, USDA, was supported in 1975 by an allocation of approximately $250 million of which only $2 to 3 million was available for funding extramural projects. Grants are used to support and supplement the in-house mission, and only if by so doing, it will be more efficient and less costly than conducting the research in-house. No detailed review of this type of granting was made as this appeared to be research directed by in-house staff and more nearly approximated NIH contractual research.

REVIEW SYSTEMS IN OTHER COUNTRIES

The NIH Grants Peer Review Study Team has not had an opportunity to study foreign systems for the review of research proposals in a careful and systematic manner. However, many individuals on the NIH staff have had firsthand experience or contact with foreign review systems. It is significant that there is not available an up-to-date and comprehensive collection of information concerning these foreign systems. In our judgment, such an exchange of information among the research administrators who are primarily responsible for these functions would be useful; information and documentation concerning review philosophies and procedures should be distributed widely. This might be an appropriate activity for the Fogarty International Center to sponsor.

The members of the NIH Grants Peer Review Study Team have had available a 1974 report prepared by Dr. Kevin O'Brien of the National Health and Medical Research Council of Australia. The Australian Government had increased the amount of funds made available to the Council at that time and it was considered opportune for a study to be made of medical research administration in other countries. Dr. O'Brien, under a World Health Organization fellowship, visited those institutions "most experienced in research administration in North America, the United Kingdom and Europe..." His 156-page report entitled "The Organization and Administration of Medical Research" served as an important resource. (20) The Study Team is not aware of any other systematic study of peer review policy and procedures in various countries; such surveys would provide useful information.

In discussing the basic principles of central research organizations, Dr. O'Brien commented that "It was clear from my study that no perfect system has yet been developed for reviewing research grants—although the NIH system is about as just and as accurate as any procedure can be which depends in the last analysis on human judgment."

Furthermore, in describing the peer review system at NIH, the report states that "The procedures followed in reviewing what are known as traditional research grants were instituted first and constitute the most thoroughly established, the most rigorously followed, the easiest to understand, probably the most widely admired of the activities included in the external programs of the National Institutes of Health... The procedures worked out....constitute a very significant administrative invention..."

It is also noteworthy that this foreign biomedical scientist felt that "it may be well to point out that years of experience have given the scientific community in the United States a respect for the ability of the staff of the Institutes which it may not have had in the beginning. This respect and trust provides a firm foundation for giving a greater degree of responsibility to experienced staff members in the early stages of formulation and review of complex proposals in the new program areas."

CANADA

Since 1969, when the Government Organization Act was passed, the Medical Research Council of Canada has been a separate department reporting to Parliament through the Minister of National Health and Welfare. The Medical Research Council, however, operates outside the framework of the Department of National Health and Welfare. The Council differs from NIH and the Medical Research Council in the United Kingdom in that the Council does not have laboratories of its own and does not conduct research directly.

In Canada, voluntary agencies provide approximately 20 percent of the grant funds that support biomedical research; a modest amount of funds is available from Provincial Governments; the central Federal Government provides the major source of support.

The evaluation and review of research proposals, as conducted by the Medical Research Council, are very similar to the NIH referral and initial review group system. Heavy reliance is placed upon assessment by peer reviewers. Applications submitted to the Council are referred to one, and sometimes two, of its standing committees. Committee members who serve without remuneration are drawn from medical school faculties. Frequently, proposals are referred to outside reviewers who are not members of one of the Council committees. Each application, along with the outside opinions, is discussed in detail by the committee. The assessment includes scientific merit, significance of the proposed research, competence and background of the investigator, previous publications, and the appropriateness of the requested budget. Numerical ratings are assigned. The evaluations and ratings of all applications are reviewed by an Executive Committee and then forwarded to the Council for final action. Some particular aspects of these research grant awards are of special interest: under these awards principal investigators may not receive any remuneration and no funds are provided for overhead.

The staff of the Medical Research Council, the Secretariat, is responsible for the actual conduct of the review and approval procedures. Each year there are three competitions for project grant support. Approximately 800 applications may be received for each of these cycles. Senior staff of the Medical Research Council Secretariat, along with scientific consultants, assign each application to the most appropriate of the seventeen grants committees or, as noted previously, to more than one committee. The assignment of applications is done primarily "by title" of the project and takes into account the competence of individual committee members. In addition to assisting in the assignment of applications, the consultants also suggest outside reviewers to whom each application could be sent for comment. Shortly thereafter, each committee chairman receives a list of applications assigned to his committee and a list of suggested reviewers. The committee chairman may suggest reassignment to a different committee or changes in outside reviewers. The applications are then sent out for review. The committee chairman is also responsible for the assignment of each application to two or more members of his committee; they will conduct an intensive review of the proposal and will initiate the discussions at the committee meeting. Each committee member may receive betweeen ten and twenty such applications for in-depth review. Although each member of the committee does not receive a complete copy of every application, each member does receive an abbreviated version of every application assigned to his committee.

Application books are distributed to committee members approximately four weeks before their meetings in Ottawa. Each committee meeting lasts about two days but this depends upon the number of applications to be considered. Applications are discussed individually and in-depth. Each reviewer is required to assign a numerical rating to every application; after the meeting these ratings are averaged and listed in order of scientific merit for presentation to the Council.

The Council rarely alters the recommendations of the Grants Committees. The Council does take into account budget allocations, the abrupt withdrawal of research support, and other factors. Budget allocations are made by support mechanisms (fellowships, grants, developmental grants) several months before each competition. From time to time special projects are identified as priority areas, e.g., prenatal diagnosis of genetic diseases, therapeutic trial of human growth hormone.

In recent years the Council, the Secretariat, and the biomedical research community have been reexamining these procedures. Some have alleged that one grants committee is much more critical and rigorous than others. Committee members who have been transferred from one committee to another have apparently not noted such differences. At times the same application has been deliberately assigned to more than one Grants Committee—the differences in ratings and recommended budgets are said to have been minimal. Studies have been performed to test the effect of eliminating the highest and lowest ratings of committee members. Thus, repeated efforts have been made to test the objectivity and the discrimination of the Canadian peer review system. Concerns continue to be expressed, however, especially as funds are not available for the support of all meritorious research.

UNITED KINGDOM

In the United Kingdom, medical research is supported in several ways. The primary routes involve the universities and medical schools, the health departments, and the Medical Research Council. The Council has had a special role in supporting promising research projects and in fostering cooperation among the universities, the biomedical professions, and the Government. The impact of position papers such as the Rothschild Report is not entirely clear but increased "targeting" and mission orientation seem apparent.

The Medical Research Council has a variety of functions and has established several advisory boards and committees. Review procedures are administered by a Secretariat in the Medical Research Council headquarters. A health scientist administrator (Scientific Administrative Officer) and a committee secretary are assigned to each Grants Committee. The main support device is the project grant. These grants finance research on a well-defined project that can usually be completed within a three-year period. Other special types of grants for equipment, travel, etc. are also available. Most applications are received from medical schools, universities, and hospitals. Upon receipt, each application is reviewed by the health scientist administrator to be certain that the application is appropriate. Each application is then submitted to one of four Grants Committees depending upon the content of the proposal. The Committee is responsible for assessment of scientific merit, appropriateness of the budget, importance of the questions being investigated, etc.

The Secretariat staff assigns applications to individual members who review the proposal in-depth and lead the discussion. Each committee member, however, is asked to rate each application. Often opinions are obtained from outside consultants and referees and these are made available to committee members. Each committee member votes a priority score and these are subsequently averaged.

The scientific evaluation performed by each of the four Grants Committees is purported to be independent of the overall availability of funds. Thus, a deliberate attempt is made to attain uniformity and consistency based primarily upon considerations of scientific merit and opportunity. Applications that score very high are awarded without referral to a second body, a Research Board—unless they are unusually expensive. Similarly, proposals that are judged to be quite poor are declined immediately. Thus, in these circumstances, the Grants Committee has the final say in approval or disapproval of applications but in most circumstances applications are further reviewed by a Research Board.

Applications reviewed by the committees are interdigitated and listed in priority order and referred to one of two Research Boards. The Biological Research Board and the Clinical Research Board have responsibility for the allocation of awards among applications and they take into account factors such as cost effectiveness, other sources of support, recurrent expenses, and program relevance.

SWEDEN

The Swedish Medical Research Council was organized in 1945 as the governmental agency primarily responsible for the support of biomedical research. One section of the Council deals with general medicine while another, composed chiefly of armed forces representatives, deals with military medicine.

The major instrument for research support is the project grant. Proposals are submitted by individual scientists and these are reviewed for scientific and technical merit by discipline-oriented committees. There are eleven such committees and each committee has approximately five members. In certain areas, deemed to be of high program relevance, special groups or committees are established, e.g., alcoholism, drugs, and occupational medicine.

The scope of the activities of the Swedish Medical Resarch Council is a reflection of the country's sophisticated medical research status. Approximately fifteen research units are supported by the Council; in these units both tenured and non-tenured faculty appointments are supported. The Council also supports fellowships in basic and clinical research and much emphasis has been given to bridging these two areas of investigation. A rather substantial portion of Medical Research Council support is allocated to clinical investigation.

Although not directly related to peer review activities, it is of interest to note that the Council supports planning activities through the use of extramural consultants as well as information activities (equivalent to MEDLARS in the U.S.), animal resources, and studies leading to new modalities in prognosis and therapy.

SWITZERLAND

The Federal agency in Switzerland that supports biomedical research is the Swiss National Science Foundation. Actually this is a private foundation but it receives the major portion of its funds from the government. Aside from the biological and medical sciences, the Foundation also covers the humanities and physics. There is a Council for each area.

Within the Foundation, the Biological and Medical Sciences Council has twelve members, most of whom are university professors. The members serve four-year, rotating terms; the Council meets frequently—ten to twelve times per year. Most of the support of medical research is channeled through units that are attached to institutions. As at NIH, applications for support are reviewed by a review group system. In fact, the Council serves this role. Site visits may be conducted by members of the Council and it appears to be almost routine to request evaluations from outside reviewers. Although the size of the country makes the performance of site visits quite reasonable, other problems do arise: the availability of reviewers is limited and such reviewers are often competing for grant funds at the same time.

The Foundation has two deadlines each year for the submisison of applications; approximately 500 applications are received in the biomedical area each year. Each application is rated and then listed in "priority order" by the Council. Final decisions on grant applications are made by the Swiss National Science Foundation sitting as a committee of the whole. In general, projects are not funded for more than three years at a time. Grants may provide for salaries, equipment, and consumable supplies. Virtually all of the grantees are employees of universities.

A relatively modest amount of funds is reserved to support research professorships at the universities and a small budget is also provided for pre- and postdoctoral research fellowships.

CONCLUSION

Thus, it can be seen that most other U. S. Government agencies and many of the western countries have some sort of a review system which utilizes persons with specific expertise as reviewers although none of the systems is identical to the dual review system used by NIH.

STUDIES OF PEER REVIEW SYSTEMS

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THE GRANTS PEER REVIEW SYSTEM OF NIH

CURRENT PEER REVIEW SYSTEM - DESCRIPTION

In fulfilling its mission, one of the functions of the National Institutes of Health is to provide for the control of human disease through the support of biomedical research projects selected for scientific merit and relevance to health problems. Because of the magnitude, diversity, and complexity of the NIH research mission and the desire to obtain the best advice possible, NIH draws on the national pool of scientists actually engaged in research to assist and participate with NIH in the selection of the best research projects to implement a biomedical research program of the highest quality.

The NIH peer review system is based upon two sequential levels of review, referred to as the "dual review system." The first level involves panels of experts, generally established along lines of scientific disciplines, such as biochemistry, pharmacology, physiology, etc. The scientific panels, called Initial Review Groups (IRGs) consist of fifteen to twenty members and have, as their primary function, the review and evaluation for scientific merit of the research grant applications submitted to NIH for consideration of support. The NIH receives annually approximately 15,000 applications requesting support, which must be reviewed by these panels.

The second level of review is carried out by the statutorily mandated National Advisory Council/Boards 1/ of the Bureaus, Institutes and Divisions (BIDs) of NIH which award grants. It is the responsibility of the Council to review the appropriateness of the technical merit review recommendations made by the Initital Review Groups and to make a final review for scientific merit. The Council must also make recommendations as to program relevance and priority. With only very rare exceptions, the BID cannot make an award unless the Council recommends an application for approval. On the other hand, a BID is not required to award a grant simply because an application receives a favorable Council recommendation. The Council also has the broader function of advising on the overall program of the particular BID as well as the legal responsibility for recommending which projects are to be awarded.

The dual review system permits separation of the assessment of projects for scientific and technical merit from subsequent policy decisions concerning programmatic, scientific areas in which projects will be supported and the level of resources to be allocated to those areas. By such a separation, a more objective evaluation is obtained than would result from a single level of review. The dual review system, because of its advisory character, provides the responsible

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1/ The National Cancer Institute and the National Library of Medicine have Advisory Boards. All of these advisory bodies will be referred to as Councils.

federal officials with the best advice possible in regard to both scientific and societal values and needs. A brief description of the procedures involved in NIH peer review follows.

Research grant and all other grant applications prepared by investigators are submitted through their institutions to the Division of Research Grants (DRG), which serves as the central receipt point for all of the applications of the BIDs. The DRG reviews each application for relevance to the overall NIH mission and, if acceptable, assigns it to the appropriate IRG for scientific merit review and to the appropriate BID for consideration of an award.

The IRGs meet three times yearly to review the applications assigned to them. IRGs are composed primarily of nonfederal scientists selected by NIH for their competence in the particular scientific areas necessary for the critical review of applications. In advance of the meeting, the Executive Secretary of the IRG studies all of the applications assigned to his/her group and obtains any additional information deemed necessary for the review from the principal investigator or applicant-institution. Each application is assigned to two or more members of the IRG, each of whom prepares, in advance of the meeting, a detailed critique of the applications assigned and who lead the discussion of these applications at the IRG meeting. Six to eight weeks in advance of the meeting, every member of an IRG receives a complete set of the applications scheduled for review at that particular meeting. In addition to the applications for which they have been designated as discussion leaders, the members are asked to become familiar with the remaining applications. During the six to eight week period prior to the meeting, the members of the IRG often contact the Executive Secretary for additional information, if needed, for the review of a particular application. In certain instances, this information can be obtained only by visiting the investigator(s) at his/her institution. The specific details of a project site visit have been described by Merritt and Eaves in their article entitled "Site visits for the review of grant applications to the National Institutes of Health: Views of an applicant and a scientist administrator." (1)

Each application is discussed individually at the IRG meeting. It is the responsibility of the IRG to review applications on the basis of technical merit and scientific significance, which includes: 1) an assessment of the importance of the proposed research problem; 2) the novelty and originality of the approach; 3) the training, experience and research competence or promise of the investigator(s); 4) the adequacy of the experimental design; 5) the suitability of the facilities; and 6) the appropriateness of the requested budget relative to the work proposed. The recommendation of the IRG is made by majority vote of the members. If recommended for approval, the application is assigned a technical merit priority rating by each individual IRG member, "1" being the most meritorious and "5" the least meritorious. The arithmetic average of these individual scores is multiplied by 100 and this numerical rating becomes the IRG's "raw" priority score. An IRG may also vote to *recommend disapproval of an* application or to defer it for further information and later recommendation. Following the meeting a summary statement is prepared for each application recommended for approval or disapproval; the summary ["pink sheet" in the case of DRG initial review groups (study sections)] contains the recommendation, the priority score, if the application has been favorably recommended and a summation of the reasons for the IRG's recommendation. This statement is forwarded to the appropriate BID for the next level of review—consideration by that unit's National Advisory Council.

In general, the National Advisory Councils also meet three times yearly for approximately three days, and each Council member receives, in advance, copies of all summary statements releveant to the BID for which they are advisory. Copies of the application are also available to the Council members, if specifically requested. The membership of the National Advisory Councils is set by law. They usually consist of twelve or more members, including some who are scientists and others who are lay community leaders interested in health areas relevant to the program areas of the particular BID.

Following the above actions by the various peer review gorups, the BID staff makes its program decisions on funding. After the investigator is notified of the recommendation of the National Advisory Council, he/she may request the BID to send a summary of the comments which led to the action taken on the application. In the case of disapproved applications, or approved applications which are not awarded, these comments can be very helpful if the investigator wishes to submit a revised application. The procedure to be followed in submitting a revised application is described in the instructions which are included in the research grant applicaton kit.

It is interesting to note that the cost of this review process, from the time an applicatoin is received to the making of an award, is a very small percentage of the amount of funds awarded. In fiscal year 1976, approximately $1.4 billion were awarded for competing and noncompeting applications, including indirect costs. The cost of the peer review process is approximately $15,800,000. This represents about one percent of the total funds awarded.

A detailed description of the peer review system is provided in Appendix C.

CURRENT PEER REVIEW SYSTEM—DESCRIPTION

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SEPARATION OF REVIEW AND PROGRAM FUNCTIONS

The Grants Peer Review Study Team unanimously and strongly endorses the principle of separation of review and program staff functions at NIH. In some respects it has been in effect at NIH almost since the beginning of the extramural grants program. The respect and reputation of the Initial Review Group or "'study section' apparatus," under the jurisdiction of the Division of Research Grants, is a testimonial to this principle.

The Division of Research Grants (DRG) conducts initial scientific and technical merit review of regular research grants* or the 11 NIH institutes, the National Library of Medicine, the Division of Research Resources, and several other segments of the PHS. It is a separate organizational entity within NIH, subservient to none of the BIDs it services. Organizationally, it is accountable only to the Director, NIH. Implicit in this arrangement is the fact that the review of regular research grants is under the management of separate DRG professional staff which has no responsibility for the management of grant programs. Funding decisions are made at the program level within the BIDs.

There are philosophical and practical reasons for this arrangement. The arrangement provides a buffer to prevent emotional or political considerations from entering into the decision-making process. Centralized peer review offers the advantage of greater efficiency in utilizing consultants to review the very large number of applications received each year. Unnecessary duplication of consultant manpower is avoided because applications can be clustered into circumscribed areas of science. Moreover, having the review apparatus centralized in one organizational locus allows for monitoring and surveillance and fosters the development of uniform review practices and procedures.

The NIH has a wide variety of grant activities. Whereas the regular research grants, fellowships, and the research career development award applications are reviewed by the DRG study sections, applications for many of the other grant programs such as program projects and centers are reviewed by the BIDs. Over the past few years, there has been a trend within the BIDs to emulate the DRG system. Thus, the National Heart, Lung, and Blood Institute has created its own peer review system to evaluate the many special grant applications. Other institutes are attempting to do the same. For example, the National Cancer Institute has a centralized peer review system for the review of program project and center grant applications. This pattern is gaining momentum at NIH because of the growth of certain programs and the recognition that the DRG model offers many advantages. The DRG model is especially attractive since the reviews being conducted in the institutes involve support mechanisms which have been subject to some criticism in the past, including large grants which are complex, involve large amounts of money, and must be subjected to thorough peer review to match that carried out by the study sections.

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*The term "regular research grants" is used to designate investigator initiated research grants coded as R01s, as distinguished from program project, center, or other special, multipurpose type of grants.

Because of the very special nature of these Institute initiated programs, it is important that there be good coordination, good planning, and good communication among Institute staff to accomplish effective, critical, and reliable review. It is important that staff carrying out reviews understand the objectives and the goals, not only of the Institute but of the particular initiative under consideration; they must also be thoroughly conversant with review policies and procedures. This review responsiblity should be placed in the hands of a staff that can apply uniform standards of peer review for the support mechanisms under its jurisdiction. At the same time, the review staff must be privy to program plans as they develop and mature in order to fully grasp the intent and background of the initiatives. It is equally important that program staff responsible for developing the initiatives and managing the awards and contracts after they are made, be given the opportunity to explain such initiatives to review committees. What emerges from this brief portrayal is a dichotomy of professional staff functions: the function related to review and the function related to program management. As in the case of the DRG-BID relationship for the regular research grants program, the professional staff functions within a BID should be kept distinct and separate.

In addition to some of the reasons already cited, one of the major advantages of this separation is to avoid prejudice and partiality or circumstances in which there may appear to be undue emotional or intellectual commitment to a particular approach. For example, even the most well-intentioned and virtuous program manager is suspect when he authors a Request for Application, selects the consultants to review the responses, chairs the review meeting, tallies the votes, writes the summary statements, selects the applications to be awarded, and serves as the program administrator. The Study Team believes that such situations must be prevented; the credibility of award decisions must be carefully guarded. To tnis end, the establishment of a peer review system within an Institute, in the hands of a professional staff detached from financial involvement in program development and management, offers the best preventive remedy to insure the continued well-being and integrity of initial peer review. In order to maintain confidence in the system and to assure adherence to the principle of separation of review and program staff functions, continued overview is necessary at both the BID and NIH levels.

The separation of review and program staff responsibilities has been demonstrated to be not only workable but wise. It has increased the objectivity and the credibility of the entire peer review system and this, in turn, accounts for its durability and the confidence in the system that presently exists.

THE ROLE OF PEER REVIEW IN DECISION-MAKING BY NIH

Although the primary focus of its efforts has been on the assessment of scientific and technical merit, the Study Team believes it is useful to reflect upon the role of peer review in the broader contexts of decision-making at NIH. Since the very inception of extramural programs at NIH peer review processes have often played a central role in many phases of decision-making. These processes have been utilized in program reviews, in determining program relevance and priorities, in setting or resetting program balance, and in the planning and evaluation of programs.

The Initial Review Groups (IRGs) take part in one of the first and probably the most important steps of the aforementioned activities: the identification of projects that are meritorious and worthy of support, using scientific merit as a primary basis for judgment.

The Institute Advisory Councils and Boards play a major role in considerations having to do with program relevance and program balance. They give advice and guidance in the identification of projects which will provide for a balanced program and which will bear significant relevance to the program objectives and missions of the specific Institutes. In making their judgments and recommendations, the Councils are mindful of the funds available to the Institutes, and the recommendations they make are based on their best judgment for the appropriate use of available funds.

On a continuing or intermittent basis, individual consultants or groups of consultants are assembled as task forces, advisory committees, and commissions, and (using such mechanisms as workshops, symposia, or regularly planned meetings) they provide useful information and suggestions to Institute staff on program planning and program evaluation. These consultants provide advice and counsel regarding research program and training areas where such information is needed, and they assess the appropriate timing for the initiation of programs to translate research knowledge into clinical application.

Traditionally, NIH has always relied heavily on the input received from its advisory groups. Although program decisions on funding, whether for program balance, program relevance or priority, or for other considerations are the ultimate responsibility of staff, nevertheless, the good judgment provided by members of peer review groups in their capacities as IRG members, Council members, or *ad hoc* consultants has formed a basis for a close partnership and trust between consultants and staff.

Strenuous efforts are made to insure that consultants of the highest quality and capability are selected. Yet there is a need to insure geographic and institutional balance as well as female, minority, and youth representation. It is also recognized that the individuals selected as IRG and Council members frequently compete for NIH funds to support their own work; this is a natural consequence of a review system that rightly places such importance on selecting reviewers of outstanding competence. The advice of such experts is considered essential and, therefore, steps have been taken to avoid conflicts of interest.

Some of the problems confronting advisory groups, and which affect their recommendations in review, have to do with their own perceptions of the appropriate balance between targeted and basic research, the balance between funds to be allocated for contract versus grant-supported activities, and the balance between project support or other support of large programs or centers. For example, in one Institute, the Council has the specific, legislatively mandated responsibility to provide advice concerning the percentage of funds to be used for contract supported research. In recent years, the Advisory Councils and Boards have become very much concerned with the balance of funds available for traditional investigator-initiated research projects and that available for the larger mechanisms of support, i.e., program projects, research centers, and other special purpose, multidisciplinary grants. In some instances they have recommended a ceiling for each type of support. Thus, the impact of the various advisory committees on such decisions is increasing. The changing character of the times, the limited availability of funds, considerations of accountability, the increased participation of public members, and the increasing visibility given to advisory committee recommendations will continue to contribute to a changing pattern of decision-making.

With all of these factors and influences weighing upon NIH decision-making, the assessment of quality and merit must be maintained as the consideration of overriding importance.

PERCEPTIONS OF THE NIH GRANTS PEER REVIEW PROCESS

PERCEPTIONS OF THE NIH GRANTS PEER REVIEW PROCESS

I. INTERACTION OF STUDY TEAM WITH THE SCIENTIFIC AND LAY PUBLIC

From the inception of the activies of the Grants Peer Review Study Team (GPRST), it was clear that there was a need to obtain the perceptions of individuals other than NIH staff about the NIH peer review system. Opinions were actively solicited from several groups namely, the scientific public, the general public, and those who participate in the peer review process as members of Initial Review Groups and Advisory Councils. The interactions are summarized below:

1. Letter Responses from the Scientific Community and the Public

A formal announcement of the establishment of the NIH-GPRST appeared in the August 8, 1975, issue of the NIH Guide for Grants and Contracts and in the September 4, 1975, issue of the Federal Register (Appendix A). Included in the announcements was a statement that the Study Team would be pleased to receive written comments, views, and relevant information from the scientific and general public concerning their perceptions of the NIH peer review system. Subsequently, in order to increase the response, a new call was made at the time the Study Team announced that it would hold open public hearings. This new call was announced in the December 8, 1975, issue of the Federal Register. In addition, a memorandum was mailed to a total of 30,000 grantee-investigators, disapproved applicant-investigators, those whose applications were approved but not funded, potential investigators, scientific associations, public interest groups, key officials of interested institutions (including minority and small schools) scientific women's organizations, scientific newsletters, and all others on the mailing list for the NIH Guide for Grants and Contracts (Appendix B). Within several months, approximately 1,500 letters were received. The letters came from a wide variety of individuals. Many respondents identified themselves as grantees, as "disappointed" applicants whose requests had been disapproved or approved but not funded, or as present or former Advisory Council or Initial Review Group members. Most spoke for themselves while some indicated that their letters represented the views of a group or a specific organization.

Although many individuals strongly endorsed the present NIH review system and pleaded that it not be "tampered with", many others were critical or made suggestions for specific changes. The letters expressed the respondents' views on a broad range of issues, and a great variety of subjects were discussed. However, review of the 1,500 letters indicated that four issues were discussed most frequently:

1. The selection of initial review group members, including concerns such as bias and cronyism.

2. Concerns over the impact of the Freedom of Information and Privacy Acts on the quality of scientific review.

3. The quality and availability of summary statements.

4. The need for a mechanism to permit appeals, in regard to the recommendations of peer review groups, as well as assignments of applications to Initial Review Groups and/or Institutes.

Other issues were addressed, but to a lesser extent. These included the quality of the review of large grants, the workload of Initial Review Groups, release of priority scores, and problems of support for minority and women investigators.

Other important elements expressed in the letters were suggestions for improving or modifying the current NIH review system. Many suggestions were presented in detail, indicating concern based on knowledge and experience and were worthy of consideration.

In reading the letters, the GPRST members were impressed by the serious and thoughtful nature of the opinions expressed. The perceptions of the Ctudy Team were considerably enhanced by the review of the letters, and its subsequent deliberations were influenced by the views offered therein.

From a careful reading of the letters, it became clear to the Study Team, however, that many statements made, in good faith, reflected a lack of knowledge of the NIH peer review system as well as of the specific steps involved in processing a grant application. For this reason, it became especially important to prepare a detailed description of the entire peer review process as part of the activities of the Study Team (Appendix C).

So that NIH may fully benefit from comments contained in the letters, a detailed analysis of them is being prepared by non-NIH consultant-contractors. The contents of the letters will be indexed by subject and then summarized. Relevant and representative excerpts will be quoted. All suggestions for changing or revising the current NIH peer review system will also be indexed and summarized. Background information on respondents which will be helpful in evaluating the comments-their status as approved or "disapproved" research grant applicants, their experience on NIH peer review groups, and other factors-will be presented, if available. This study is currently in process and results are expected to be available in late 1977, and will be included in Phase II of the NIH Grants Peer Review Study Team Report.

2. Open Public Hearings for the Scientific and Lay Community

The Study Team recognized the value of the letters in providing input from the community but it felt that more direct interaction was also desirable. Accordingly, it was agreed that open public hearings would be an important and very useful means of eliciting information and recommendations from concerned scientists and others affected by the grants peer review process. Public hearings were held as one day sessions during February 1976 in government facilities in each of three geographic locations. The meetings were held in Chicago, Illinois, San Francisco, California, and Bethesda Maryland. The Study Team designated a panel of its members to attend all three hearings. The panelists routinely asked the speakers questions designed to elicit clarification of of, or elaboration upon, key points but made no attempt to debate issues with those testifying or to correct clearcut inaccuracies in the written and oral comments offered. Twenty to thirty formal 10 minute presentations were heard at each hearing. However, additional speakers were heard from the floor, and the panel remained in session until all comments were received. Despite the limited number of presentations (69), the Study Team felt that much useful information was gained. A list of speakers, their institutional affiliations, and transcripts of the Hearings testimony are available in the files of the GPRST. Copies of the transcripts may be obtained by addressing the Freedom of Information Officer, Office of the Director, NIH, Bethesda, Maryland 20014.

The speakers were advised in advance that the GPRST was interested in their views on the following topics:

1. Adequacy of the total review system, e.g.,
    a. The effectiveness of the system in serving and responding to societal needs and expectations for biomedical research on disease-related problems.
    b. The effectiveness of the system in assisting in maintenance of a strong, high quality national biomedical science base.
    c. The extent to which the system assists in meeting the best standards of public accountability for expenditure of public funds.

2. Adequacy of the initial scientific review.

3. Adequacy of the Council review.

4. Adequacy of the priority rating system.

5. Impact of the Privacy Act of 1974.

6. Impact of the Freedom of Information Act, as amended in 1974.

7. Impact of the Federal Advisory Committee Act.

8. Recommendations as to how the present Grants Peer Review System can be improved.

Each speaker was asked to present a written summary, or a general outline or statement in full, in advance of the hearing.

Examination of the testimony presented at the hearings indicated that the speakers represented 55 different institutions and 11 organizations. Some individuals traveled a considerable distance at their own expense to attend the hearing at which they were scheduled to speak. An effort was made by the Study Team to attain a geographic spread of institutional representation among the speakers. Generally, however, the speakers were accepted on a "first come, first served" basis, as their requests were received in the GPRST office. A special effort was made, however, to encourage presentations from minority schools and, indeed, four such institutions were represented. The president of a women's scientific organization responded with the views of women scientists. Two legal officers presented the views of their organizations relating to legal aspects of the peer review system, and other speakers addressed these issues as well.

A number of speakers expressed total support for the NIH system, but others spoke to aspects of the system about which they had some concern. In general, the speakers who expressed concerns over aspects of the peer review process addressed the same issues addressed in the letter responses: (a) the process by which members of IRGs and Advisory Councils/Boards are selected, including related comments regarding cronyism and the "buddy" system, (b) the impact of the Freedom of Information and Privacy Acts on the quality of scientific review, (c) the quality and availability of summary statements, and (d) the need for a peer review appeals mechanism. Several speakers spoke eloquently about the need to support young investigators, and the need to support unorthodox or high risk research. A few speakers presented interesting suggestions for changing certain aspects of the peer review system such as the creation of a permanent office at NIH to plan and conduct experiments in regard to the peer review process.

It is important to remember that the hearings were held to obtain the views of those who participate in, or are otherwise affected by, the peer review process so that the perceptions of the external community could be reflected in the thinking of the Study Team. This was also the case in the letter responses from the external community. Similarly, it became clear from the hearings that there are misconceptions and misunderstandings thoughout the external community concerning the operation of the NIH peer review system, and it became doubly clear that not only was a description of the process necessary, but that it should be widely disseminated.

The testimony presented at the hearings is presently being analyzed and evaluated in detail in a manner similar to that for the letter responses, and will be included in Phase II of this report to be completed in late 1977.

3. Survey of Initial Review Group and Advisory Council/Board Members

The third group of individuals from whom comments and opinions were sought by the Study Team was made up of those who participate in the peer review process as members of Initial Review Groups and Advisory Councils. This was accomplished by means of a survey questionnaire designed and developed by a subcommittee of the parent Study Team with the aid of a consultant survey research specialist and other non-NIH consultants. The purpose of the survey was to obtain assessments of the existing grant application peer review system and suggestions for its improvement from active participants in the system--the current members of NIH grant review groups and advisory councils. More specifically it was planned that the survey focus on the reactions of the group members in three major areas:

1. Evaluation of the current NIH grant peer review system.
2. Assessment of the impact of recent changes and of potential future changes on the quality and efficiency of the NIH grants peer review system.
3. Suggestions for improving the existing system.

Four other basic decisions were made regarding the survey:

1. The anonymity of all respondents was to be preserved in order to obtain valid answers,
2. The respondents would be encouraged to supply comments in order to expand their responses, or to include concerns omitted from the questionnaire,
3. The survey would include all current members of the peer review system and not be limited to a sample of such members, and
4. Only one survey instrument would be developed even though not all questions would be equally applicable to each of the three types of review groups surveyed — the Division of Research Grants Initial Review Groups, the BID Initial Review Groups, and the National Advisory Council or Board of each BID.

Analysis and Evaluation of the Survey of Initial Review Groups and Advisory Councils

A detailed report of the statistical findings of this analysis appears in Supplement A. A summary of the major findings of the survey is attached (Tab A). Additionally, a detailed comprehensive report of the survey, including analysis and evaluation of the comments section, is being prepared and will be available at a later date.

A total of 1,354 questionnaires was distributed to 12 Advisory Councils/Boards*, 51 DRG-IRGs, and 24 Institute IRGs during their meetings in November-February 1976. The questionnaires were given, not only to the regularly appointed members of the peer review groups, but also to liaison members representing Federal agencies and to those ad hoc consultants attending those meetings whom the Executive Secretary considered to be knowledgeable concerning the review process.** The overall response rate was exceptionally high, 94%, attesting to the interest and cooperation of the peer review group members.

In examining and commenting on the survey responses, the differences in mission and composition of the Initial Review Groups and the Advisory Council/Boards must be kept in mind. It should also be remembered that the DRG-IRGs generally review the traditional investigator-initiated research grant applications; the Institute IRGs generally review the special purpose type grant applications, usually the large program project or center grants, and other types of grants submitted in response to specially announced program needs of an Institute. Another fact to be borne in mind is that the IRGs are the "producers of a product," namely, a summary statement ("pink" sheet) and a priority score for the application, whereas the Advisory Councils/Boards are the "users of the product." Such differences in function may well lead to different appraisals of the adequacy of a particular product or NIH procedure.

Approximately one-half of the 1,274 initial review group and National Advisory Councils/Boards members who completed the 1975-76 Peer Review Survey questionnaire also wrote one or more remarks in the ample space provided for comments. A slightly higher proportion—51 percent—of National Advisory Council/Board members than of initial review group members—49 percent—made comments.

Because it was judged important to utilize fully the comments and suggestions made by this experienced and knowledgeable group, the Survey Subcommittee of the Grants Peer Review Study Team decided to sponsor an in-depth analysis. This work is currently in its initial stages and will be completed in 1977. However, in order to enable the Study Team to benefit from the comments during its deliberations, two major projects were undertaken:

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*Members of the Board of Regents of the National Library of Medicine were not surveyed.

**Thirteen percent of the respondents were liaison members or special reviewers.

1. The comments were transcribed to a computerized data base and the resultant computer listings of the full text of each comment, categorized by applicable survey question, were circulated.
2. Four analytical reports on issues of special interest to the Study Team were completed. These reports are presented in Supplement B.

Tab A, as mentioned above, presents the major findings of the statistical analysis of the survey.

TAB A

MAJOR FINDINGS

The current NIH system for the review of research grant applications was overwhelmingly endorsed by those individuals who were members of NIH review or advisory groups at the time of the survey. More than one-half characterized the system as a whole as "excellent" and over 95 percent rated it as either "excellent" or "good." Equally high ratings were given the general fairness and lack of bias found in the operation of the system.

The general satisfaction with the current NIH review system was reflected in the lack of enthusiasm expressed for recent changes and for potential future changes in the system arising from Congressional and public concerns with the openness of Government processes. Of 16 such changes, only four were perceived by more than half of the review group members as having either a "very favorable" or a "favorable" effect on the quality and efficiency of the NIH grant review system.

Even less enthusiasm was shown for the 20 suggested improvements listed in the questionnaire. One suggestion--recommending a study on how to reduce the time lag between application and final NIH action--received the endorsement of three-fourths of all review group members. None of the other 19 suggested changes received the support of a majority.

The members of the DRG Study Sections were generally the most favorably disposed of the three types of review groups toward the current NIH review system and the members of the National Advisory Councils/Boards were generally the least favorably inclined. Even so, about 90 percent of the National Advisory Council/Board members designated the overall NIH review system and its lack of bias as excellent or good. The greater degree of satisfaction of DRG Study Section members was also evidenced by the fact that they were less favorable than both the other two review groups in their responses to 14 of the 20 suggested improvements listed in the questionnaire.

The survey findings showed that members of the three types of review groups varied in certain characteristics and that some of these differences reflected differences in functions among the groups. On the average, DRG Study Section members had the least prior experience with the NIH peer review system, had the greatest proportion engaged currently in research activities or as principal investigators on NIH research grants, had the smallest proportion in clinical activities, were the youngest, and had the smallest percentage of women members. Members of the National Advisory Councils/Boards differed the most from DRG Study Section members in many characteristics while Institute IRG members frequently fell between the two other groups. In some instances, the characteristics of Institute IRG and National Advisory Council/Board members were similar--over one-third were engaged in clinical activities and one-fourth were women (Tables 4, 5 and 6).

Delays occurring in recent years in making appointments to National Advisory Councils/Boards were reflected in the finding that almost one-half of the members of these advisory groups were serving the first year of their current appointment. In contrast, Institute IRG members were relatively evenly spread among the four years while about one-third of DRG Study Section members were first-year appointees (Table 4).

Assessment of the Current NIH System

All parts of the NIH review system did not receive the high level of approval elicited by the questions on the overall system and its general fairness. Frequently, responses on specific features were less favorable than the answers given on more general aspects. Nonetheless, over one-half of the review group members assigned a rating of either excellent or good to the 48 questions on the current system which are appropriate for this kind of an analysis. /1 That is, each aspect of the current NIH review system covered by one of the 48 questions received a favorable rating from more than a majority. In fact, 33 of the 48 questions on the current system were rated favorably by at least 80 percent of the review group members. The greater satisfaction of the DRG Study Section members is clearly evident, as is the relatively more critical assessment of the National Advisory Council/Board members:

Distribution of questions by the proportion of members responding excellent/good or the equivalent

Percent of members responding favorably	Number of questions			
	All groups	Initial Review Groups		National Advisory Councils/ Boards
		DRG Study Sections	Institute	
Total	48	48	48	48
Under 50 percent	0	0	0	0
50 to 59 percent	1	1	0	1
60 to 69 percent	2	3	6	10
70 to 79 percent	12	9	11	8
80 to 89 percent	14	11	16	20
90 percent and over	19	24	15	9

Persons accepting appointments and actively participating in the NIH research grant review system may be expected to be favorably disposed toward that system as well as knowledgeable about its policies and procedures. In

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/1 Seven of the items in Part II of the questionnaire were omitted from this analysis--Question 4n; Questions 10b, d, f, h and k; and Question 11.

analyzing the ratings [12] assigned by the review group members to specific aspects of the system, it is difficult to discern negative reactions. With the exception of three questions, at least 70 percent of the respondents judged the current system as either excellent or good. However, it is possible to group the replies by the differing levels of support given specific parts of the current NIH research grant review system (Table 1).

The strongest endorsements, those aspects of the current system approved by at least 90 percent of the review group members, included:

. . . Lack of general bias in the NIH grant peer review system as well as in the performance of NIH staff, peer review group members, and the peer review system procedures. The responses of DRG Study Section members were consistently the most favorable and those of National Advisory Council/Board members consistently the least favorable (Table 7).

. . . Lack of bias against minorities, young investigators or women in the review of applications in recent years. Again, the DRG Study Section members were the most favorable and the National Advisory Council/Board members the least favorable. More bias was perceived on the other side of the coin--favoritism toward these groups, particularly young scientists (Table 7).

. . . The overall adequacy of the current NIH review in general and for the initial review of traditional research project (R01) grant applications (Table 8).

. . . The adequacy of the current review performed for the scientific and technical quality of new grants and for the capability of research investigators (Table 9).

. . . The value of site visits to the review process and the quality of site visits. In a shift from their usual ranking, Institute IRG and National Advisory Council/Board members were more enthusiastic about the value of site visits than the DRG Study Section members who conduct such visits much less frequently than Institute IRG members as part of their reviews (Table 10).

. . . The performance of review group members as measured by the quality of the discussion of applications and their behavior during the review process. Once more, the DRG Study Section members were the most favorable (Table 13).

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[12] The questions on the current system permit the selection in most instances of one of the following: excellent, good, fair, poor, very poor, or no opinion. For some questions on bias, the choices are: none, insignificant, moderate, significant, very significant, and no opinion. Percentages are computed on a base which excludes "no opinion" and all nonresponses to that question.

. . . The scientific and technical members' qualifications and performance. The assessments made by the members of the National Advisory Councils/Boards, which include lay or public members, were less favorable than the opinions of the members of both initial review groups which are entirely composed of such scientific and technical members (Table 13).

. . . NIH staff qualifications and performance in administering the peer review system, both the executive secretaries of the initial review groups and all other staff (Table 13).

At the other extreme, the least support was given two of the current NIH restrictions governing applicant notification. The lowest rating (56 percent favorable) was given the present requirement which prohibits informing the applicant of the priority score assigned by the initial review group and the third lowest score (69 percent favorable) was given the requirement which delays informing the applicant of the overall initial review group recommendations until completion of the final review by the National Advisory Council/Board (Table 1).

The weakest endorsements, those approved by less than 80 percent of all review group members, included:

. . . Some bias toward "cronyism" although rejected by over 70 percent of all review group members who perceived no signs or only insignificant signs of such favoritism in the review of applications (Table 7). About 20 percent reported observing moderate amounts of such a practice and almost 10 percent reported observing significant or very significant amounts. Sharp differences existed in the perceptions of "cronyism" among the review groups. Only 5 percent of the DRG Study Section members perceived significant or very significant evidence of "cronyism" compared with 12 percent of the Institute IRG members and 24 percent of the National Advisory Council/Board members. Within each review group, women members and the younger scientists were the most critical.

. . . The review of program project and center grant applications was judged less adequate by all three types of review groups than the highly-rated initial review of traditional research project grants. However, the degree of satisfaction varied among the groups. Over 85 percent of the members of the Institute IRGs, who perform these reviews and are most intimately familiar with the process, rated such reviews as either excellent or good. Almost as high a proportion of National Advisory Council/Board members, who use the reviews in their deliberations, indicated satisfaction. The lowest rate of approval--73 percent--was by the DRG Study Section members, many of whom may not be familiar with this type of review as is evidenced by their unusually high 35 percent no opinion/no response rate on this question (Table 8).

TABLE 1. ASSESSMENTS OF THE CURRENT NIH RESEARCH GRANT REVIEW SYSTEM

Grouped by the proportion of all review group members responding excellent/good or the equivalent

QUESTIONS ON THE CURRENT SYSTEM (PART II OF QUESTIONNAIRE)	PERCENT
A. 90 percent or more responding excellent/good (19 items)	
Q1. How adequate is the review currently performed for:	
a. The scientific/technical quality of new grants	96*
c. Capability of research investigators	92
Q2. How adequate in general, is the initial review of traditional research project (RO1) grant applications?	97*
Q4. How would you rate the:	
a. Value of site visits to the review process	90
b. Quality of site visits	91
d. Quality of discussion of applications during review sessions	91
Q5. How satisfied are you, on the average, with:	
b. The QUALIFICATIONS of scientific/technical members of the review and advisory groups?	95*
c. The PERFORMANCE of scientific/technical members of the review and advisory groups?	95*
Q6c. How would you describe the behavior of other team members during the review process?	92
Q7. How would you rate the qualifications and performance of NIH staff who administer the peer review system?	
a. Executive secretaries of initial review groups	94*
b. All other staff	91
Q8. Taking all significant factors into account, how would you characterize the NIH grant peer review system in toto?	96*
Q9. On the basis of YOUR OWN experience as a member of a review group, how would you rate the fairness and lack of bias of the NIH grant peer review system?	
a. In general	95*
b. With respect to performance of peer review group members	93*
c. With respect to NIH staff performance	94*
d. With respect to peer review system procedures	94*
Q10. Please indicate the extent of significant biases, if any, you have observed in the review of applications in recent years (percent responding none/insignificant)	
a. Age (anti-youth)	91*
c. Race (anti-minority)	98*
e. Sex (anti-female)	95*
B. 80 to 89 percent responding excellent/good (14 items)	
Q1. How adequate is the review currently performed for:	
d. Resources and environment required for research	81
f. Suitability of the requested period of budget support	83
Q4. How would you rate the:	
f. Effectiveness of the system of approval/disapproval/deferral	88
g. Effectiveness of the 1.0-5.0 priority system	82
i. Quality of summary statements (pink sheets)	86

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* At least half of those indicating an opinion selected "excellent" or "none".

TABLE 1. ASSESSMENTS OF THE CURRENT NIH RESEARCH GRANT REVIEW SYSTEM - CONT'D

QUESTIONS ON THE CURRENT SYSTEM (PART II OF QUESTIONNAIRE)	PERCENT
j. Procedures regarding possible conflict of interest	81
k. Review of competing renewal applications and their progress reports	89
l. Review of supplemental applications	85
m. Review of revised (amended) applications	87
Q5. How satisfied are you, on the average, with:	
a. The effectiveness with which the NIH grant peer review system uses your talents and expertise	88
f. NIH administrative policies and procedures for peer review	84
Q6a. How would you describe your overall working conditions during the review process?	87
Q10. Please indicate the extent of significant biases, if any, you have observed in the review applications in recent years (percent responding none/insignificant)	
g. Innovative ideas (anti)	84*
j. NIH staff interference	89*
C. 70 to 79 percent responding excellent/good (12 items)	
Q1. How adequate is the review currently performed for:	
b. Collaborative arrangements essential to research	79
e. Appropriateness of requested budget	78
Q3. How adequate, in general, is the initial review of program project and center grant applications?	78/1
Q4. How would you rate the:	
c. Time available for site visits	71
e. Time available for review	71
h. Effectiveness of the 100-500 priority score ranking system	77/1
Q5. How satisfied are you, on the average, with:	
d. The qualifications and performance of public members of advisory groups	70/2
e. The adequacy of the selection process for peer review group members	73/1
Q6. How would you describe your working conditions during the review process?	
b. In terms of the adequacy of the facilities provided	77
d. Enabling you to function at effective physical/mental levels (impact of fatigue)	71
Q10i. Please indicate the extent of cronyism, if any, you have observed in the review of applications in recent years (percent responding none/insignificant)	72
Q12c. Indicate your opinion of the present NIH requirement that the research grant applicant NOT be informed of individual reviewers' comments	[illegible]
D. 50 to 69 percent responding excellent/good (3 items)	
Q1g. How adequate is the review currently performed for relevance of proposed research to NIH programs	67
Q12. Indicate your opinion of the present NIH requirement that research grant applicants NOT be informed of	
a. Overall Initial Review Group recommendations until after final Council/Board review	69
b. Initial Review Group priority scores	56

* At least half of those indicating an opinion selected "excellent" or "none".

/1 No opinion/no response rate was 17 to 25 percent.
/2 No opinion/no response rate was 42 percent.

. . . The adequacy of the review for program relevance, which is the primary responsibility of the National Advisory Councils/Boards, was assessed as excellent or good by 67 percent of the review group members, the second lowest rating for a specific feature of the current review system. DRG Study Section members, who are instructed not to concern themselves with this aspect of the review, were the least favorable while Institute IRG members, some of whom may consider this as part of their own function, were the most favorable (Table 9). Within all three types of review groups, members currently engaged in clinical activities were less satisfied than those not so engaged.

. . . The reviews for the appropriateness of the requested budget and for collaborative arrangements essential to research were perceived more favorably by members of both types of initial review groups, who are responsible for these functions, than by the members of the National Advisory Councils/Boards (Table 9).

. . . The time available for site visits was rated least favorably by the members of the Institute IRGs, the review group that is most concerned with such visits (Table 10). However, only 5 percent of this group rated the time available as either poor or very poor.

. . . The 100 to 500 priority score ranking system apparently posed a problem of understanding to members of both types of initial review groups as a high percentage did not respond to the question on the effectiveness of this system. However, about three-fourths of all those responding from each of the three review groups considered the system as either excellent or good (Table 10).

. . . Current restrictions on applicant notification had sizeable opposition. The present NIH requirement forbidding research grant applicants to be informed of their priority scores was perceived as a poor or very poor practice by almost one-third of all review group members. Approximately one-fifth of the members also objected to the other two current requirements listed--not informing the applicant of the overall IRG recommendation until after final Council/Board review and not making individual reviewers' comments available to applicants. However, a majority of all review group members regarded all three requirements as either excellent or good (Table 11).

. . . The time available for review appeared to be an area of some dissatisfaction. About 70 percent of all review group members rated the time available as either excellent or good. However, only 6 percent felt sufficiently dissatisfied to rate the time available for review as either poor or very poor. The greatest dissatisfaction was shown by the members of the National Advisory Councils/Boards (Table 12).

. . . Working conditions as measured by the facilities provided and the provisions made to enable the members to function at effective physical and mental levels were felt to be much less satisfactory by DRG Study Section members than by the members of the other two review groups. Members of the National Advisory Councils/Boards were very well satisfied (Table 12).

. . . The selection process for peer review group members was not heartily endorsed--less than three-fourths of all review members rated the adequacy of this process as excellent or good. In addition, the substantial no opinion/no response rates among all three types of review groups implies that many members may feel uninformed (Table 13).

. . . The public members' adequacy and performance was rated as excellent or good by slightly over two-thirds of the National Advisory Council/Board members. A very high proportion of initial review group members supplied no opinion as they probably had no basis for forming any opinions concerning the qualifications or performance of the public members who are not represented on their groups (Table 13). Among the National Advisory Council/Board members, opinions appeared sharply divided in such a way as to lead to the conclusion that the public members were rated less favorably by the scientific members than by themselves.

Changes in the Current System

The review group members were generally negative in their reactions to the suggested changes for "opening up" the present NIH research grant system. Only four of the 16 recent or potential changes listed in the questionnaire were perceived by a majority of members as probably having a "very favorable" or "favorable" impact on the quality and efficiency of the system (Table 2). In addition, one recently adopted change--opening review group sessions to the public when not reviewing applications or budgets--was considered by a majority as having either a favorable impact or "no impact."

Of the four suggestions for "opening up" the system which the majority of the survey respondents considered desirable, three involved increasing the access to the summary statements (pink sheets). Making these statements available upon request to review group members and, without the priority score, to principal investigators drew considerable support; less enthusiasm was shown toward making the summary statements available to principal investigators with the priority score. The fourth suggestion receiving majority approval dealt with making the critiques of the individual reviewers available to the principal investigators upon request (with the reviewers anonymous).

TABLE 2. ASSESSMENTS OF THE IMPACT OF RECENT AND POTENTIAL CHANGES

Grouped by the proportion of all review group members responding very favorable/favorable

Questions on the impact of recent and potential changes (Part III of questionnaire)		Percent responding	
		Very favorable/ favorable	No effect
A. More than 50 percent responding very favorable/favorable (4 items)			
Q6	Making summary statements (pink sheets) available upon request to:		
	a. Review group members	83	12
	b. The principal investigator, without the priority score	79	8
	c. The principal investigator, with the priority score	60	7
Q7a	Making the individual reviewers' critiques available to the principal investigator upon request, with the reviewers anonymous	53	6
B. 20 to 49 percent responding very favorable/favorable (3 items)			
*Q1	Release of new, funded grant applications to the public	24	25
*Q3	Opening the sessions of the Initial Review Groups and Advisory Councils/Boards to the public when not reviewing applications or budgets	34	37
Q6d	Making summary statements (pink sheets) available upon request to officials of the applicant institution	37	10
C. Under 20 percent responding very favorable/favorable (9 items)			
*Q2	Public release of all renewal/continuation applications (funded or not)	13	18
Q5	For those sessions which review grant applications		
	a. Opening Initial Review Group sessions to applicant investigators	7	4
	b. Opening IRG sessions to officials of applicant institutions	9	8
	c. Opening Initial Review Group sessions to the public	5	5
	d. Opening Advisory Council/Board Sessions to applicant investigators	8	7
	e. Opening Advisory Council/Board sessions to officials of applicant institutions	11	9
	f. Opening Advisory Council/Board sessions to the public	7	8
Q6e	Making summary statements (pink sheets) available upon request to the public	12	13
Q7b	Making the individual reviewers' critiques available to the principal investigator upon request, with the reviewers identified	5	2

* Practices already in effect at the time of the survey.

Even the three recently instituted changes toward "opening up" the system were expected to affect the NIH peer review system adversely. Approximately 70 percent of all review group members anticipated negative effects from the public release of renewal or continuation applications; one-half expected unfavorable effects from the public release of new, funded grant applications; and 30 percent regarded opening the sessions to the public when not reviewing either applications or budgets as having a very unfavorable or unfavorable impact.

Over 60 percent of the review group members were not aware of all three of the recently adopted changes toward "opening up" the NIH review system and a substantial 17 percent were not aware of any of them (Table 14). Such a lack of awareness on the part of the review group members raises the question of adequate communication. A mitigating factor may be the fact that many members are serving in the first year of their current appointment.

Another part of the questionnaire listed 20 suggested improvements and requested the review group members to rate these suggestions. The ratings designated by the review group members indicated a general satisfaction with the current system. /3 Of the 20 suggestions, only one--dealing with reducing delays--received strong support. Over three-fourths of all members endorsed a suggestion that a special study be made on how to shorten the time between the submission of an application and its subsequent funding or denial of funding. None of the other suggestions received approval from a majority of review group members (Table 3).

Seven of the 20 suggested improvements were considered as either excellent or very good by from 29 to 43 percent of the review group members. Of these seven suggestions, three dealt with reducing the workload of initial review group members. The method given the most support was to increase the number of review groups; over two-thirds rated this suggestion as fair or better. However, the other two methods suggested--reducing the maximum term of appointment to three years, and staff screening and rejection of inadequate applications--were rated poor or very poor by more than a majority.

Differences among the three types of review groups became apparent in the variations in their support of some of the suggested improvements. Although all three groups considered the special study on reducing delays as desirable, a majority of DRG Study Section members did not rate any of the other 19 suggestions as excellent or very good. However, a majority of Institute IRG members thought encouraging review group members to recommend site visits was an excellent or very good idea, as did a majority of National

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/3 An analysis of the Comments section of the questionnaire may indicate substantial support for improvements which were not listed in the questionnaire. Such an analysis will appear in Part II of the Report to the Survey Subcommittee.

TABLE 3. ASSESSMENTS OF SUGGESTED IMPROVEMENTS

Ranked by the proportion of all review group members responding excellent/very good

Questions on suggested improvements (Part IV of questionnaire)	Percent responding	
	Excellent/ very good	Fair
Q17. A special study on how to shorten the time between submission of an application and funding or denial of funding	77	14
Q2a. Reduce the workload of IRG members by increasing the number of review groups	43/1	26/1
Q9. Encourage review group members to recommend site visits	40/1	22/1
Q6. Provide a specific structured format for individual reviewers' reports	33	24
Q13. Permit an applicant to suggest reviewers in case of appeal only	31	26
Q8. Increase the number of site visits	30/1	24/1
Q2c. Reduce the workload of IRG members by reducing the maximum term of appointment to 3 years	29/1	18/1
Q2d. Reduce the workload of IRG members by staff screening and rejection of inadequate applications	29	13
Q5. Provide specific review format with weighted rating procedure for review criteria	27	22
Q11. Delay the appointment of a retiring member to another group for longer than the one-year moratorium now required between appointments	25/1	19/1
Q3. Increase the assigned reviewers for each proposal from to 3 in each IRG	25	20
Q2b. Reduce the workload of IRG members by increasing the number of members in each group	23	22
Q4. Curtail or omit discussion of those applications judged by assigned reviewers to be either outstanding or very poor	23	15
Q12. Permit an applicant to suggest reviewers	20	18
Q14. Include scientific/technical members employed by for-profit organizations on grant review groups	19	21
Q7. Decrease the number of site visits	16/1	14/1
Q15. More frequent review group meetings	14	15
Q10. Discontinue site visits on research project (R01) applications	6	7
Q16. Less frequent review group meetings	3	12
Q1. Increase the workload of IRG members	2	5

/1 No opinion/no response rate was 15 to 27 percent.

Advisory Council/Board members. In addition, more than one-half of the National Advisory Council/Board members thought it would be an excellent or very good idea to have a specific structured format for individual reviewer's reports, to have a specific review format with weighted rating procedures for the review criteria, and to reduce the workload of initial review group members by increasing the number of review groups (Table 16).

More detailed discussions of specific aspects of the current NIH review system and of suggested changes or improvements are given in the last three sections of this report. In addition, certain significant aspects of the current NIH review system which have not been mentioned in this section because they did not fall into either the "most favored" or "least favored" groups are dealt with in some detail. These include all those aspects of the current system that were rated as excellent/good (or the equivalent) by from 80 to 89 percent of all review group members (Table 1).

MAINTENANCE AND IMPROVEMENT OF QUALITY OF REVIEW

PEER REVIEW GROUP MEMBERSHIP

SELECTION OF INITIAL REVIEW GROUP MEMBERS

It is clear, from the public hearings, letters, and discussions with other interested persons that: (1) the selection of initial review group (IRG) members is a sensitive issue of considerable interest; (2) there is a strong desire for more formal input from the scientific community and/or public regarding nominations for membership to IRGs; and (3) the method by which members are selected and the many considerations involved therein have neither been adequately described nor are generally understood.

I. BACKGROUND

The areas of biomedical science for which a particular initial group has review responsibility are briefly described in the HEW publication "NIH Public Advisory Groups, Authority, Structure, Function, Members". (1) A detailed and specific description of the review responsibility of each IRG in the Division of Research Grants (DRG) is further provided in the Referral Guide related to Study Sections (IRGs). (2) The latter is updated annually to reflect advances in the field and/or administrative changes in review responsibility of an IRG. In addition to providing technical merit review of applications, members also have the responsibility of surveying, as scientific leaders, the status and needs of research in their respective fields.

II. REVIEW OF CURRENT PROCEDURES, RESOURCES, AND SELECTION CONSIDERATIONS

A. Current Procedures

Formal nominations for membership to an initial review group originate with the Executive Secretary. One candidate is nominated for each position and the final selection is made by the Director, NIH, or his designee. The factors considered and resources available to an Executive Secretary in making these selections are discussed in subsequent sections.

A series of well defined procedures are followed, and review of all nominations is conducted at several levels within NIH and HEW before a nomination is approved and an invitation to IRG membership is extended by the Director or agency head.*

1. The Executive Secretary initiates a nomination by forwarding the "Request for Approval of Nominees for Public Advisory Committees" (Form HEW-532, Attachment 1) to the Committee Management Office(CMO) of the particular Bureau, Institute, or Division (BID). The information provided includes a description of the candidate's special expertise, type of qualifications needed for the position, proposed term of membership, and the nominee's curriculum vitae.

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*The Director, National Cancer Institute, has special separate statutory authority to establish advisory committees, 42 U.S.C. 282.

2. At the same time the Executive Secretary also forwards the list of nominees and the above information to the Chief of Extramural Programs (or other designated person) of the Institutes for which the IRG reviews substantial numbers of applications. The covering memorandum notifies the relevant Institutes of the proposed appointment and provides opportunity for informing the Executive Secretary of any serious questions or concerns regarding the nominations.

3. After the nomination(s) has been reviewed by the BID Director or his/her designee, the Committee Management Office (CMO) initiates an Availability Request Clearance which is forwarded, through the CMO of the Office of the Director, NIH, to the relevant office at HEW. There, information concerning previous membership on HEW committees and terms of office is provided and considered. The total time required for this action is six to eight weeks.

4. Prior to the issuance of an invitation, the Executive Secretary has a final opportunity to decide whether the specific nominee should be asked to serve on the IRG, since during the period between nomination and approval, membership requirements and/or conditions relating to the nominee's availability for service may have changed.

5. The BID Director or his/her designee issues the invitation to serve on the specific IRG for a specific term, in a letter which states the review and other functions of the IRG and describes the responsibilities incumbent upon such membership. Accompanying the letter of invitation are the required forms, including the Confidential Statement of Employment and Financial Interests (HEW-474, Appendix E-2) for determining potential conflict of interest.

6. When the letter of acceptance and the completed HEW-474 form are received, the latter is reviewed prior to final appointment.

7. Nominations for service as chairperson of an IRG require, in addition to the data supplied for regular membership, a memorandum describing the nominee's special qualifications for this position. Since prior service with the IRG is desirable, this selection is frequently made by the Executive Secretary from among the current members. In other cases, the nomination may involve reappointment of a former member of the IRG after a lapse of service and justification is required for this action.

B. Resources

The nomination and selection of members is a major professional responsibility of the Executive Secretary, involving long-range planning, suggestions, and information from many different sources. It is a matter of continuous research and concern. Numerous resources are available and are constantly drawn upon by the Executive Secretary in developing suitable nominations for membership:

1. The Executive Secretary's own knowledge of the discipline and of the scientists who are making significant research contributions to the field.

    a. Review of NIH applicant and grantee files and summary statements as well as curricula vita and publications of investigators.

    b. Reading of the major scientific journals and publications in the field.

    c. Attending relevant professional meetings. These provide a valuable additional way of keeping informed of the most significant new studies in the field and of identifying the investigators who are doing the research. Knowledge is thus gained of the interests and expertise of potential future IRG members. Such meetings also provide opportunity for consultation with eminent investigators who are not, or have not been, members of the IRG group.

2. Service of investigators as "special reviewers"; for example, (a) as ad hoc consultants at a regular IRG meeting when additional or special expertise is needed; (b) as members of a project site visit team; or (c) by providing written collateral opinions on request.

3. Solicitation of names of outstanding investigators in a given area from former and current IRG members and other leaders in the field.

4. Lists of women and minority scientists.

5. Consultation with scientific and professional staff of the various BIDs who have wide contacts with the scientists in a given field.

6. Attending, as observer, the review of applications by other agencies—especially those having review panels in related disciplines.

From a combination of such sources the Executive Secretary develops a large backlog of names of well-qualified scientists as potential IRG members. Nominees are selected from this list.

C. Nomination and Selection Considerations

The criteria, guidelines, and conditions for selection and appointment of members of NIH Initial Grant Review Groups have undergone changes and evolution since the peer review system was established. These are outlined in the memorandum to BID Directors dated 6/23/72 (Appendix D). More recently, proposed rules governing the composition of peer review groups (as published in the Federal Register, Vol. 41, #61, p. 12987) (Appendix F-2) are in the process of being codified.

In preparing a slate of nominees (usually three to five, one for each vacancy) as replacements and/or additions to the membership, a combination of specific criteria and conditions for appointment must be considered by the Executive Secretary relative to the present composition of the IRG. Some of the major considerations involved are listed below to emphasize their importance and interrelationships.

1. Areas and disciplines to be covered. Since the primary function of any IRG is to provide expert scientific merit review of the highest quality, the first and major consideration in selection of members is the particular area(s) of expertise needed. This relates not only to immediate or current needs but also to areas in which deficiencies will occur as other members complete their terms of service. In general, investigators with broad knowledge of the field, as well as excellent expertise in a specific subspecialty, are sought. The importance and the complexity of this function are formidable. Consideration of appropriate nominees for each opening requires not only the selection of people in specific disciplines but a search for and recognition of expertise in specific subdisciplinary areas. A balance of expertise in such areas in the entire committee is also required as is an understanding of the way in which the appointment of a specific individual would affect the spectrum of expertise—not only initially—but when other members retire. Specifically, this exercise requires this assessment over future years to assure the necessary balance and coverage for each new appointment proposed to the Initial Review Group. This function makes the greatest demands on the professionalism of the Executive Secretary and is perhaps the single most important determinant as to whether the IRG will operate efficiently with the appropriate balance and expertise.

2. Previous service, if any, on scientific merit review groups of NIH and/or other agencies. Before a nomination is sent forward it must be determined that the nominee is not currently a member of an HEW committee and has not served on such for at least one year prior to the expected date of appointment. It is the policy not to make reappointments to the same IRG group. Exceptions to these policies are rare and are made only in special cases. For example: (a) to appoint a chairperson of an IRG, since previous service on that review committee is valuable experience in undertaking this special responsibility; and (b) when the number of experts in a given field or subspecialty is so limited as to require reappointment. Other rare exceptions may involve dual appointments, when the nominee is, for example, serving on another NIH Advisory Committee such as the Board of Scientific Counselors of an Institute which reviews only intramural research activities.

3. Institutional Representation on Initial Review Groups. Individuals nominated for membership must be drawn from institutions not currently represented on the IRG. Two individuals from the same institution may not be appointed to a given IRG.

4. Geographic Distribution of Membership. An attempt is made to have representation from all areas of the U. S. and to maintain a balance with regard to geographic distribution in the nomination of new members. Again, this must take into account the location of current members and the time when each individual's appointment ends.

5. Minority Representation. Through the various resources mentioned above, there is a continual search both for ethnic group and women scientists who are experts in specific subspecialties that need to be represented. Equitable representation of such qualified individuals on the IRG must be an important consideration in selecting nominations; it is, again, also related to current composition of the IRG and the individual members' terms of service.

6. Term of Appointment. Although the appointments to an IRG are usually for a four-year term, members are sometimes nominated to serve for only two to three years for various reasons. In this regard, consideration is given to such matters as the nominee's other professional commitments, future plans for sabbatical leave, etc. A further factor is the number of members scheduled to complete their terms of service in a given year, to maintain a desirable balance between experienced and new members.

7. Personal Qualifications. Less tangible, but nevertheless essential additional considerations relate to judgment concerning: (a) the nominee's ability for functioning effectively and cooperatively with the group and with the Executive Secretary; and (b) the nominee's conscientiousness in fulfilling the obligations incumbent upon IRG membership, and for carrying out the objectives of the Peer Review System.

III. SUGGESTIONS FOR CHANGES IN THE SELECTION OF IRG MEMBERS

The Study Team's interactions with the scientific community and with the general public left little doubt that many individuals are concerned about how IRG members are—or appear to be—selected. Many of those who commented on the present grants peer review system expressed the belief that the pool of scientists from which NIH draws reviewers is unnecessarily and inappropriately narrow and that many qualified individuals consistently are overlooked. Others expressed uncertainty and uneasiness about the seemingly closed nature of the reviewer selection process and urged both more openness and broader participation by the scientific and lay communities. It is also apparent from the public hearings, the comments, and the letters received that most individuals do not know how IRG members are selected, nor are they aware of the care and wide variety of resources currently employed in making these selections. Thus the process described above seems to be one aspect of the NIH grants peer review system that is in need of clarification and could benefit from input from knowledgeable interested groups and/or individuals who are not now regularly consulted.

Given the widespread concern about so fundamental an element of the grants peer review system, the Study Team concluded that the NIH should make the details of its reviewer selection procedures more broadly known and, in addition, should provide a mechanism whereby individuals and organizations can suggest candidates for IRG membership.

The current practice of obtaining suggestions, for "replacements" or for future membership, from retiring or current members, has been challenged from various sources, on the basis that IRG members tend to perpetuate membership from their own institutions or from among their colleagues. The perception is that members choose their successors, or that, basically, they control who is invited to serve on IRGs. This misimpression can and should be corrected without delay, by: (1) much wider dissemination of information regarding the range of resources currently drawn upon by Executive Secretaries of IRGs; and (2) publication of the criteria and conditions for selection (as outlined in Appendix D).

Another suggestion has been made that current and retiring members should be specifically excluded from providing advice regarding replacements while they are still, officially, members of an IRG. Adoption of such a policy would, hopefully, aid in correcting the perception that a "buddy system" of selection exists. However, while this may be desirable for "cosmetic" reasons, it would weaken the continued effort to improve the quality of review and to obtain advice from highly knowledgeable sources. Those who are serving on a particular IRG are often in the best position of all potential consultants to know what the gaps in technical expertise are or are likely to be, and to provide suggestions as to the best qualified scientists to fill these needs. It would, therefore, be in the best interest of the maintenance of the quality of the Peer Review System to continue to seek suggestions from current, retiring, and former IRG members as well as from a wide variety of other sources.

It has been suggested that an IRG Executive Secretary provide a first and second nomination for each position to be filled. Presently, only one individual is nominated for each appointment to be made.

The possible advantages of having two nominees are: (a) if the first-choice nominee is unable to accept appointment there would be less delay in filling the position; (b) there could be more involvement of other interested parties such as staff of the various Bureaus, Institutes, and Divisions (BIDs) of NIH; and (c) the second nominee may help dispel the impression held by many that IRG and Council members personally select their own replacements and that IRGs and Councils are closed, select groups of self-perpetuating cronies representing and distributing research dollars only to the largest and most prestigious institutions.

Disadvantages are: (a) since the combination of conditions which must be met presently in the selection of only one suitable, available, and well-qualified expert per vacancy is a difficult and time-consuming undertaking, this would compound the problem; (b) since the scientific expertise, the location or university affiliation etc., of any one nominee is significantly related to other nominees in terms of scientific requirements and other conditions to be met, it cannot be considered in isolation from all other nominations or acceptances.

It is felt that the disadvantages outweigh the advantages of providing two nominations simultaneously for each position since this would add significantly to the background work of each Executive Secretary while giving little added flexibility. If the pool of known qualified investigators can be substantially increased by other methods, it appears that the current process of selection of one nominee for each vacancy should be continued.

Finally, the suggestion has been made that investigators may submit names of possible consultants who, they feel, are particularly knowledgeable in the technical aspects of the research proposed. This procedure is presently in effect but obviously it is not generally known. It is clear that a method must be devised to implement this procedure in a formal manner.

RECOMMENDATIONS:

1. THAT, IN ORDER TO MAKE THE REVIEWER SELECTION PROCESS BETTER AND MORE WIDELY UNDERSTOOD AND TO REGULARIZE THE WAYS IN WHICH THE SCIENTIFIC AND LAY COMMUNITIES CONTRIBUTE INFORMATION IN SUPPORT OF IT, THE NIH SHOULD PUBLISH PERIODICALLY (e.g., ANNUALLY) AN ANNOUNCEMENT WHICH CALLS ATTENTION TO THE UPCOMING VACANCIES ON IRGs ASSOCIATED WITH THE GRANTS PEER REVIEW SYSTEM AND WHICH INVITES SUGGESTIONS REGARDING CANDIDATES FOR SPECIFIC IRGs.

   The announcement should appear in the *Federal Register* and the NIH Guide to Grants and Contracts, and brief notices should be sent to scientific journals, such as *Science*, etc. Also, the announcement should be circulated among those individuals, institutions, scientific societies, and other organizations which request that their names be maintained on a mailing list for this purpose.

   The announcement should include the following items of information about:

   (a) the NIH grants peer review system:

   - a brief statement of the role and importance of an IRG in the peer review process;
   - a description of the nature and scope of an IRG member's duties, responsibilities, and workload;
   - a description of the key steps in the reviewer selection process;

- a listing of the professional characteristics that NIH looks for in selecting IRG members; and
- a discussion of the other factors which are taken into consideration in selecting IRG members (e.g., geographical and institutional spread, representation of ethnic minorities and women, and other committee management limitations).

(b) each specific IRG:

- its function;
- the number of vacancies to be filled;
- the areas of scientific and technical expertise to be represented by the new members.

Reference to the regularly published roster of NIH Public Advisory Groups should be included. (1)

It should be stated that potential respondents must document the qualifications of each individual suggested (attachment of curriculum vitae and bibliography, etc.) and specify the IRG(s) for which the individual is to be considered, if suggestions are to be considered. There also should be a clear statement that NIH, while willing to consider all submissions, reserves the right to make final selections without presenting public justifications or accounting specifically either to those who suggest candidates or to those who are suggested.

It is further recommended that responsibility for coordinating the preparation and distibution of the announcements and for providing suggested names to the appropriate IRG Executive Secretary should be centralized within the Office of the Director, NIH.

In addition, in order to assess its impact, the effects of this new procedure should be evaluated by carefully selected criteria within a given period of time (e.g., five years after implementation).

2. THAT, IN ORDER TO PROVIDE FOR ADDITIONAL ADVICE BEYOND THAT GIVEN BY INITIAL REVIEW GROUP MEMBERS, PARTICULARLY IN REGARD TO THOSE RESEARCH GRANT APPLICATIONS IN WHICH NEW SCIENTIFIC FIELDS ARE TO BE EXPLORED OR UNIQUE PROCEDURES ARE TO BE DEVELOPED, THE NIH SHOULD IMPLEMENT A FORMAL PROCEDURE WHEREBY AN APPLICANT MAY IDENTIFY THOSE PARTICULAR ASPECTS OF THE PROPOSED RESEARCH WHICH ARE CONSIDERED TO BE UNIQUE AND TO SUGGEST POSSIBLE CONSULTANTS WHO ARE CONSIDERED TO BE LEADERS IN AND CURRENTLY KNOWLEDGEABLE ABOUT HIS/HER AREA OF RESEARCH. HOWEVER, THE DECISION AS TO WHETHER TO SEEK ADVICE FROM THE SUGGESTED EXPERT(S) OR ANY OTHER EXPERT(S) MUST REMAIN THAT OF THE NIH.
(It is suggested that such proposals should be made in a separate letter which accompanies the grant application.)

3. THAT, THE CURRENT PRACTICE OF CONSULTATION WITH PRESENT, RETIRING, AND FORMER IRG MEMBERS CONCERNING POSSIBLE NEW MEMBERS SHOULD CONTINUE TO BE BUT ONE OF A SERIES OF STEPS BY WHICH ADVICE CONCERNING POTENTIAL IRG MEMBERS IS SOUGHT FROM MANY SOURCES.

4. THAT THE CURRENT PRACTICE OF PROVIDING ONLY ONE NOMINATION FOR EACH IRG VACANCY SHOULD BE RETAINED. FURTHERMORE, ADVICE REGARDING SUCH NOMINATIONS TO DRG INITIAL REVIEW GROUPS SHOULD BE SOUGHT FROM KNOWLEDGEABLE AND INTERESTED STAFFS OF THE VARIOUS BIDs RESPONSIBLE FOR THE ADMINISTRATION OF GRANTS IN THE PARTICULAR BIOMEDICAL SCIENTIFIC AREAS.

The above recommendations, when implemented, should lead to the selection of IRG members in the future by an open, objective process which assures that, as in the past, only qualified persons participate in peer review.

SELECTION OF INITIAL REVIEW GROUP MEMBERS

References

1. NIH Public Advisory Groups: Authority, Structure, Functions, Members. (Obtainable from Committee Management Office, Building 1, Room 303, National Institutes of Health, Bethesda, Maryland 20014. Published twice annually).

2. Referrral Guide in Relation to Study Sections. Division of Research Grants, National Institues of Health.

3. DHEW Scientific Peer Review of Grant Applications and Contract Projects. Fed. Reg. 41 (61): 12987 (1976).

Attachment 1.

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
REQUEST FOR APPROVAL OF NOMINEES
FOR PUBLIC ADVISORY COMMITTEES
(See reverse for instructions)

Date Prepared

☐ **Principal**
☐ **Alternate**

Name of Companion Nominee

Name of Nominee: *(last, first, middle, prof. degrees)*

Business Title:

Home Adress:

Business Address:

Date of Birth:

Place of Birth:

Agency:

Proposed Committee

☐ **Initial Appointment**
Proposed Term:
From:
To:

☐ **Reappointment**
Proposed Term:
From:
To:

Current Term:
From:
To:

Name of Retiring Member:

Termination Date:

Sources of Recommendations:

Name	Title	Date

Special Qualifications of Nominee *(briefly describe unique qualifications)*

Type Qualifications Needed for Committee Position

Previous Membership on DHEW Committees and Terms of Office

Program Director Recommendation/Approval

Date ____________ BY: ____________

Agency Head Recommendation/Approval

Date ____________ BY: ____________

Department Committee Management Office Concurrence

Date ____________ BY: ____________

Assistant Secretary Recommendation/Approval

Date ____________ BY: ____________

Assistant to the Secretary Recommendation

Date ____________ BY: ____________

SECRETARY'S APPROVAL

Date ____________ Secretary ____________

HEW-532 (Rev. 3/72)

SELECTION OF MEMBERS OF SPECIAL INITIAL REVIEW GROUPS (SIRGs)

Generally, research grant applications are reviewed by duly constituted Initial Review Groups (IRGs). However, certain conditions (described below) preclude the use of such IRGs for the review of special types of research grant applications. In such cases, Special Initial Review Groups (SIRGs) are organized, the membership of which reflects the review needs of a particular research grant application or group of applications.

I. CURRENT PRACTICE - SPECIAL INITIAL REVIEW GROUPS

The selection of special review group consultants is based on the guidelines set forth for IRGs (1) considering major factors such as scientific competence, and expertise in a particular scientific field as well as as standing in the scientific community and other factors such as conditions which could create conflict of interest situations, representation of women and minority groups, and the geographic distribution of consultants.

1. Conditions which govern the assignment of a research grant application to an SIRG include:

a. Submission by members of IRGs of applications for which there is no other IRG with the appropriate expertise to undertake review.

b. Submission of applications of such complexity that the scientific content overlaps the review area of two or more IRGs; e.g., Program Projects, Research Centers, Clinical Centers, or Biotechnology Resource applications.

c. Submission of applications in response to BID announcements of specialized program interests; e.g., Research Fellowship Awards, Request for Applications (RFAs), or International Fellowships.

d. Submission of applications for which it is determined that there are other special review requirements.

2. Process of selection of SIRG members

The SIRG Executive Secretary reviews the application(s) in question and develops a roster of potential reviewers after consultation with the Program Director of the BID to which the application has been assigned, making use of his/her previous experience and technical insight, and that of other Executive Secretaries having comparable review experience.

Other contributing sources frequently used to obtain consultants for an SIRG include: reviewers suggested by the Principal Investigator of the application to be reviewed, individuals identified in reference citations in the application, reviewers who have completed tenure on IRGs, as well as, on occasion, current IRG members who may have unique expertise. Whenever possible, highly qualified young investigators are invited to serve along with their more senior counterparts.

An SIRG generally consists of a minimum of 5 and a maximum of 12 depending upon the size and complexity of an application as well as the number of similar proposals to be reviewed. If a project site visit is required for the review of an application, the Principal Investigator receives a copy of the SIRG roster together with other pertinent details relevant to the pending site visit. The final SIRG roster may be modified upon the request of the Principal Investigator. However, assignment of an application to an SIRG does not always insure that a project site visit will be made.

Upon completion of a specific review assignment, the SIRG is dissolved.

II. IMPACT OF LEGISLATIVE PROPOSALS

A threshold question involves what constitutes a "committee." It is clear from the above that there is a recurring need to call together groups such as SIRGs, and, while the Federal Advisory Committee Act (FACA) does not distinguish between short-term and more permanent advisory groups, evidence that has been presented in this report would allow a strong argument to be made that "special" groups formed to review a single application or group of applications are not committees for the purposes of the FACA. However, pending legislation (Senate Bill No. S 2947) would amend the definition of "advisory committee" to encompass "ad hoc" groups thus making it impossible to set up SIRGs needed to provide for the important situations described above.

III. RECOMMENDATIONS AND DISCUSSION

1. THAT IT IS ESSENTIAL THAT NIH CONTINUE TO HAVE THE FLEXIBILITY AND OPPORTUNITY TO ESTABLISH SPECIAL INITIAL REVIEW GROUPS.
2. THAT NIH AND HEW SHOULD OPPOSE LEGISLATION REGARDING AD HOC GROUPS.

The review of applications assigned to SIRGs is complex. The workload is uneven and inconsistent. At times there may be a large number of applications to be reviewed; at other times there may be only a few; but in almost all cases the applications are complex and extraordinary skills are required in organizing appropriate SIRGs. Timing is important depending on availability of consultants willing to undertake review assignments which may require an absence of two to three days away from their own institutions, frequently on short advance notice. Furthermore, a mechanism for the proper review of applications from IRG members is necessary. It is therefore essential that NIH continue to have the flexibility and opportunity to establish SIRGs freed from the formality and delay involved in establishing IRGs.

3. THAT THE ROSTER OF CONSULTANTS TO BE INCLUDED ON A SPECIAL INITIAL REVIEW GROUP BE SUBMITTED TO ALL INVESTIGATORS.

Under the present system only those principal investigators for whose projects a project site visit is planned receive the roster of consultants prior to the site visit. This proposal would provide equity for all investigators whose applications are reviewed by SIRGs.

SELECTION OF MEMBERS OF SPECIAL INITIAL REVIEW GROUPS (SIRGs)

References

1. Handbook for Executive Secretaries. Division of Research Grants, National Institutes of Health; Fourth Edition. (1976).

NOMINATION, SELECTION, AND APPOINTMENT OF ADVISORY COUNCIL MEMBERS

Each of the awarding Bureaus, Institutes, and Divisions (BIDs), has a National Advisory Council or Board to offer advice and make recommendations on matters relating to the mission and goals of the Institute it serves. The Councils and Boards are specifically charged to review applications for grants-in-aid relating to research projects, grants and awards for research training and other activities as may be defined for accomplishing the mission of the awarding BID. The Council receives recommendations on applications for grants from initial review groups, which have been charged with making the scientific and technical merit review. The Councils or Boards advise on policy matters related to the development and management of grant programs and provide final review for scientific merit and program relevance of applications for grants-in-aid. They provide periodic review and evaluation of grant program accomplishments, help to identify and resolve problems in program development, and recommend guidance for the Institutes in the context of national health programs.

As established by law, each Council and Board is advisory to the Secretary, DHEW, through the Director, NIH, and the Director of the awarding BID.* For the most part, the membership of each Council or Board consists of the Secretary, DHEW, or his designee, plus leaders in the fundamental sciences, medical sciences, education and public affairs as may be approved by the Secretary.

I. PROCEDURES AND GUIDELINES FOR SELECTION OF COUNCIL OR BOARD MEMBERS

The Public Health Service (PHS) Act (1) directs the Secretary, DHEW, to appoint members to NIH Advisory Councils in accordance with the requirements set forth by each of the sections of the Act which apply to the particular Council.** These requirements, in general, are designed to provide authorities knowledgeable in the programmatic areas within the Institute's mission, familiar with the NIH procedures, aware of the roles of diverse institutions in biomedical research, and mindful of the health needs of the American people. To provide this range of expertise, each Council consists of both scientists and public members. *Ex officio* members from other Federal agencies are appointed as required by law.

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*With the exception of the National Cancer Advisory Board which is advisory directly to the Director, NCI.

**Members of the Board of Regents of the National Library of Medicine and of the National Cancer Advisory Board are appointed by the President.

A. Criteria for Selection

1. Scientific Members

Individuals selected as scientific members are chosen from among those recognized as outstanding leaders in their fields and considered to have a broad interest in and understanding of the relationship of biomedical science to societal needs. It is expected that the Council members will have recognized achievement in a relevant field, other scientific accomplishments, and honors. The assessment of appropriate abilities and accomplishments is based primarily on the general recognition by peers in the biomedical areas as a broadly based expert in the current and past development and status of a particular field of research. In addition to superior accomplishment, the characteristics of mature judgment, balanced perspective, and objectivity are essential. Candidates must have demonstrated the capability of working effectively in a group context as committee members. Those who accept appointment are expected to accept responsibility for all work assignments.

2. Public Members

Individuals selected as public members should have a demonstrated interest and background history relevant to the program of the Institute. These individuals should have knowledge concerning the needs and aspirations of society in the areas of the mission of the Institute.

3. General Criteria and Policy for Selection of Council Members*

a. Within a given Council, representation of needed scientific disciplines or medical research specialties must permit the attainment and maintenance of a proper balance to cover the range of the mission and goals of the Institute.

b. Whenever possible, selection of candidates should reflect an equitable representation of the geographic regions of the U. S. These considerations, however, must remain subsidiary to and should not take precedence over the appointment of individuals of the highest qualifications.

c. No two members of a Council should be appointed from the same institution or organization.

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*These general criteria and policies apply only to advisory bodies appointed by officials within DHEW. Even where the appointing official is the Secretary or other DHEW official, exceptions to these criteria and policies may be permitted.

d. No new member should be appointed to the same or any other Council within a year after termination of a prior appointment. No member may serve concurrently on more than one DHEW advisory group.

e. Within the criteria, guidelines, and conditions noted above, NIH practice is to utilize the widest available pool of qualified candidates for advisory service, including equitable representation of minorities, women, and youth. Considering all these factors, primary consideration for Council nominations should be given to scientific leaders in their specific fields of competence and lay representatives of demonstrated interest and background relevant to the program of the Institute to which they have been nominated as Council members.

4. Procedures for Nominating Candidates for Advisory Councils

The general procedure is for NIH to nominate one primary and one alternate for each vacancy on an Advisory Council. (Each professional or scientific vacancy on a Council is usually limited to a specific medical or scientific need; see A-3a.) The Office of the Secretary, DHEW, may request additional nominations and/or make nominations itself. Once the Secretary, DHEW, selects a candidate to fill a Council vacancy, he sends a letter of invitation. The PHS Act directs the Secretary to appoint members to Advisory Councils in accordance with the requirements set forth by each of the sections of the Act which apply to the particular Council. A recent and most welcome addition is that all appointments to advisory committees to assist in implementing the Publc Health Service Act are required to be made without regard to political affiliation.*

II. DEFICIENCES AND/OR PROBLEMS ENCOUNTERED IN CURRENT SELECTION PRACTICES

Appointments to Councils are for overlapping terms of four years. Vacancies occur automatically at the expiration date. Unless filled in a timely fashion, a vacuum is created and the Council will function at less than optimum efficiency. Maintaining a Council at full strength is essential if it is to function effectively. At present, the NIH has no knowledge of or control over the timing of appointments of new Council members. Both delayed appointments and unfilled vacancies continue to threaten the efficient operation of the Councils. For example, there are 153 authorized Council positions. One-quarter of these terminate annually as individual terms expire. Forty-four appointments are needed to fill the vacancies for the calendar year, 1976. As of November 1, 1976, the 44 positions still remain vacant.

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*Section 1001, Public Law 94-278, Health Research and Health Service Amendments of 1976.

The PHS Act authorizes the Council to advise on policy matters related to the development and management of grant programs. It also authorizes the Council to provide a second review for scientific merit and program relevance of applications for grants-in-aid. In general, no grant can be awarded unless recommended for approval by the awarding unit's advisory council or board. Serious operational problems can and do occur when substitutes for NIH nominated candidates are made without NIH knowledge and concurrence. In such instances, the discrete balance of scientific specialities necessary to cover the range of areas of expertise required by the Institute's programs can be destroyed with the resultant effect that the Advisory Council may have no representation for one or more research areas (and other areas may have duplicate representation). Unless there is adequate and appropriate representation for each of the major areas of concern to each Institute, the Council will lack the ability to carry out its statutory responsibility with maximum effectiveness.

III. RECOMMENDATIONS AND DISCUSSION

1. THAT NIH SHOULD SEEK TO HAVE THE AUTHORITY FOR SELECTION AND APPOINTMENT OF MEMBERS OF ADVISORY COUNCILS/BOARDS DELEGATED TO THE ASSISTANT SECRETARY FOR HEALTH, DHEW.

Maintaining a Council at full strength is essential for the Council to effectively meet its responsibilities as a reviewing body for applications and advising the Institute on program priorities and balance. At present the NIH has no knowledge of or control over the selection or appointment of new Council members. Unfilled vacancies, delayed appointments, and the appointment of individuals lacking in the required expertise continue to threaten the effective operation of Advisory Councils. It is essential that someone knowledgeable of the needs of the Institutes, and the roles of and requirements for Advisory Council members, has responsibility for assuring that the total selection and appointment process proceeds in an effective and timely manner. The Assistant Secretary for Health, the highest department health official, has the background and scientific knowledge to carry out these requirements.

2. THAT THE PROCEDURE FOR NOMINATING AND SELECTING COUNCIL MEMBERS BE MORE OPEN. SCHEDULED COUNCIL VACANCIES SHOULD BE ANNOUNCED AND PUBLISHED EARLY IN THE FISCAL YEAR. THE ANNOUNCEMENT SHOULD SPECIFY THE COUNCIL INVOLVED, THE CRITERIA FOR THE SELECTION OF COUNCIL MEMBERS, THEIR DUTIES, AND TERMS OF APPOINTMENT. THE NAMES OF INDIVIDUALS SELECTED SHOULD BE PUBLISHED TOGETHER WITH THEIR QUALIFICATIONS.

The Grants Peer Review Study Team, in assessing the perceptions of the scientific and informed lay community as to the duties of and criteria for selection of Council members, became aware that there was misinformation and, in many cases, lack of knowledge as to the duties and functions of the Advisory Councils as well as the criteria and methods for the selection of Council members. Accordingly, the GPRST urges that there be wide dissemination of information as to the duties and functions of the Advisory Councils and the methods by which members are selected.

3. THAT, WHEN A SELECTION HAS BEEN MADE FOR A COUNCIL VACANCY(IES) OTHER THAN FROM NOMINATIONS SUBMITTED BY THE DIRECTOR, NIH, THE APPOINTMENT SHOULD NOT BE MADE FINAL UNTIL THE DIRECTOR, NIH, HAS HAD THE OPPORTUNITY TO COMMENT ON THE SELECTION.

Through its experience, the NIH has acquired considerable knowledge of the appropriateness of many individuals to serve on a Council and an appreciation of their effectiveness. By implementing the above procedure, the risk of appointing inappropriate or potentially ineffective members would be avoided.

NOMINATION, SELECTION, AND APPOINTMENT OF ADVISORY COUNCIL MEMBERS

References

1. Public Health Service (PHS) Act, [42 U.S.C. 241].

CONSIDERATIONS IN REGARD TO CONFLICT OF INTEREST

THE UTILIZATION IN NIH COMMITTEE MANAGEMENT OF THE FORM HEW 474 (CONFIDENTIAL STATEMENT OF EMPLOYMENT AND FINANCIAL INTERESTS)

In accordance with Civil Service Commission and HEW requirements, NIH periodically secures from each member of its advisory committees a Form HEW 474, regarding the member's employment and relevant financial interests.

I. Background:

Generally speaking, the Federal conflict-of-interest statutes make it a crime for a special Government employee (such as an advisory committee member who is not otherwise employed by the Government), except in the discharge of his/her official duties, to represent anyone else in a particular matter in which the United States is a party or has a direct and substantial interest and (1) in which he/she has at any time participated personally and substantially in the course of his/her Government employment, or (2) which is pending before the Government agency he/she serves. There is also a criminal sanction against an employee participating as part of his/her official duties in a particular matter in which, to his/her knowledge, the employee's spouse, minor child, partner, or a profit or nonprofit enterprise with which he/she is connected, has a financial interest. 1/ (Appendix E-1)

As written, the conflict-of-interest statutes simply describe the prohibited activities and the penalties involved, but do not specifically require agencies to take steps to prevent potential violations thereof. Nevertheless, although individuals are, in theory, charged with notice of the criminal laws, it is often not readily apparent that the laws apply in certain contexts. Hence, agencies do undertake to assist employees in avoiding violations of these laws. This is reflected, for example, in the HEW Document, Standards of Conduct, which discusses conflict of interest in detail, sets forth a procedure for obtaining advice on such matters, and even has a separate subpart addressed specifically to special Government employees. Another example is the procedure outlined elsewhere in this report by which NIH deals with applications from advisory committee members' own institutions or from the members themselves.

1/ It should be emphasized that this is only a general description of these statutes. For a more detailed discussion, see the HEW Standards of Conduct, 45 C.F.R. Part 73.

II. The Form HEW 474

Only the individual member normally has the basic information about his/her activities which is necessary to avoid all but the most obvious conflict-of-interest situations. A major purpose of the Form HEW 474 (Appendix E-2) is to direct the member's attention to the subject of conflict of interest and to require him/her to consider his/her own activities in light thereof. A second goal of the form is to aid agencies in identifying situations in which potential conflicts may exist.

Agencies are required to secure this form from special Government employees by Executive Order 11222 which reads in pertinent part as follows:

> "Sec. 306. Each agency shall, at the time of employment of a consultant, advisor, or other special Government employee require him to supply it with a statement of all other employment. The statement shall list the names of all corporations, companies, firms, State or local governmental organizations, research organizations and educational or other institutions in which he is serving as employee, officer, member, owner, director, trustee, adviser, or consultant. In addition, it shall list such other financial information as the appointing department or agency shall decide is relevant in the light of the duties the appointee is to perform. The appointee may, but need not, be required to reveal precise amounts of investments. The statement shall be kept current throughout the period during which the employee is on the Government rolls."

This provision has been implemented by Civil Service Commission and HEW regulations. 2/ (5 C.F.R. §735.401 et seq.; 45 C.F.R. §73.735-1201 et seq.) Among other things, these regulations prescribe the contents of the Form HEW 474; require that the form be reviewed by a high level agency official; and specify that the form be maintained in the appropriate personnel office but separate from the official personnel folder. The regulations permit agencies to make the completed forms ". . . available only as specifically authorized by the head of the operating agency . . . for good cause shown . . .," but this would not prevent full-time Federal employees (such as Executive Secretaries) from looking at the forms in connection with their official duties.

In order to implement the aforequoted portion of the Executive Order at its level, NIH has published Manual Issuances 2300-735-1 and 2300-735-2, dealing with conflicts of interest on the part of special Government employees. The former (Appendix E-3) is a one-page document which simply directs consultants and advisors to submit Forms HEW 474

2/ The HEW regulations are commonly referred to as the HEW Standards of Conduct.

and to update them on a yearly basis and requires NIH staff to issue a copy of the Standards of Conduct to all newly appointed or reappointed consultants and advisors, together with a memorandum from the Secretary, HEW, alerting HEW personnel to become familiar with the contents thereof.

As had already been indicated, the Standards of Conduct regulations require that each Form HEW 474 be reviewed by a high level official " . . . to determine whether conflicts of interest or apparent conflicts might arise from the activities reported thereon." The Standards also contain sections relating to such specific matters as professional and consultative services, writing and editing, publishing, teaching, and holding office in professional societies or State or local government.

NIH Manual Issuance 2300-735-2 is entitled "Conflict of Interest Policy for Committee Members." (Appendix E-4) It sets forth guidelines for the selection and appointment of members to avoid certain types of conflicts, but places on the committee member ". . . the primary responsibility for evaluating his financial interests or those of his family, that relate directly or indirectly to his duties." It assigns to the BID Directors responsibility for determining who shall review information on the Form HEW 474 and who shall have access to such information. The only specific responsibility given to the Executive Secretary is to remind committee members periodically about ". . . NIH and other pertinent policy regarding avoidance of conflict-of-interest situations."

Since the Executive Secretary plays such a central role in the actual operation of committees, the Director, DRG, has established a policy (Appendix E-5) for the DRG study sections which provides as follows:

> "1. Each Executive Secretary, prior to a regular study section session, shall review in [Mrs. Pollak's] office the forms 474 on file for that study section. Such notes as necessary may be made.
>
> 2. Study section members who identify themselves on Form 474 as affiliated with organizations shall not participate in the discussion or vote on an application from any organization so listed. In other words, the present policy of nonparticipation on applications from major employers shall be in effect for these organizations.
>
> 3. At the close of each study section meeting, each member must certify that he/she was absent during the discussion of matters from any and all institution or institutional systems with which he/she is affiliated. The exact wording of the certification to be signed will be developed by the Chief of the Scientific Review Branch and approved by me. As you will note in the report, this is current practice for council members.

4. Each Executive Secretary must prepare a list of applications for which he/she excused certain study section members from review. This list should give the name of each excused member under the pertinent application number. This list is to be attached to the original copy of the formal minutes and retained in the study section's official files. The duplicated copies of the minutes should contain a statement to this effect. The standard statement is to be developed by the Chief, Scientific Review Branch, for all to use."

The Grants Peer Review Study Team recognizes that it is essentially impossible for an agency such as NIH to take cognizance of sufficient information about advisory committee members to note and avoid all potential conflicts of interest. Hence, NIH justifiably places primary responsibility for avoiding these conflicts on the members themselves. Nevertheless, NIH also has a duty to see that members are informed about conflict-of-interest considerations and to take reasonable steps to help in the identification and avoidance of potential conflicts. The Form HEW 474 serves the multiple purpose of bringing the conflict-of-interest requirements to members' attention, causing them to think about these requirements in relation to their own situations, and of providing NIH with added information for use in identifying possible problems in this area.

Although the Form HEW 474 contains useful instructions, it is the Study Team's view that they are too general to fully meet NIH's needs. As a result, some IRG members interpret the instructions too narrowly and fail to list relevant information.

The Study Team endorses the approach being taken by DRG of requiring Executive Secretaries to become familiar with information on their members' Form HEW 474s. The Executive Secretary is the NIH staff person who is most knowledgeable about the backgrounds of the members and the business of the particular committee, and therefore is in the best position to anticipate and prevent potential conflicts.

III. Recommendations

Regarding Form 474:

1. THAT NIH DEVELOP DETAILED SUPPLEMENTAL INSTRUCTIONS TO BE SENT TO MEMBERS ALONG WITH THE FORM HEW 474, EMPHASIZING THAT EMPLOYMENT INCLUDES, FOR EXAMPLE, FOREIGN EMPLOYMENT, SUMMER AND PART-TIME EMPLOYMENT, MEMBERSHIP ON ADVISORY BOARDS OF ORGANIZATIONS, AND CONSULTANT APPOINTMENTS; AND THAT RELEVANT FINANCIAL INTERESTS INCLUDE SUCH THINGS AS ROYALTY AGREEMENTS WITH OR STOCK OWNERSHIP IN DOMESTIC OR FOREIGN BIOMEDICAL RESEARCH ORGANIZATIONS, COMPANIES ENGAGED PRIMARILY IN PROVIDING SERVICE TO SUCH ORGANIZATIONS, PHARMACEUTICAL COMPANIES, AND OTHER COMPANIES INVOLVED IN DRUG RESEARCH.

2. THAT NIH ADOPT A PROCEDURE UNDER WHICH FORM HEW 474 IS RETURNED TO A MEMBER AS INCOMPLETE, WHERE SUCH MEMBER MAKES NO ENTRY IN A SECTION OF THE FORM BUT DOES NOT WRITE "NONE" OR SOME EQUIVALENT IN THE SECTION.

3. THAT THE EXECUTIVE SECRETARIES OF IRGs BE GIVEN ACCESS TO THE FORMS HEW 474 OF MEMBERS, AS NEEDED.

4. THAT REVIEW OF FORMS HEW 474, AT LEAST ANNUALLY, BY EACH EXECUTIVE SECRETARY BE MANDATED THROUGHOUT NIH.

5. THAT ALL EXECUTIVE SECRETARIES BE REQUIRED PERIODICALLY TO ATTEND TRAINING SESSIONS ON EVALUATION OF CONFLICT-OF-INTEREST SITUATIONS.

IV. Invitations to Members

Under current practice, once an individual is selected to serve on an advisory committee, if the appointing official is at NIH, the Form HEW 474 will be sent out along with the invitation to membership. Where a higher level official makes the appointment, the Form HEW 474 often is not mailed to the selected individual until after the invitation is made.

While the format of the invitation letters may vary, in general, they do not make it explicit that the invitation is a contingent one, and that final appointment depends on, among other things, review of the completed Form HEW 474 to determine whether, in light of conflict-of-interest restrictions, the individual will be able to participate sufficiently in the activities of the committee to make a meaningful contribution to the work of the committee. Moreover, in most instances, once the invitation is made, the individual will not receive another formal communication indicating that his/her appointment is final. It is not surprising, therefore, that individuals would normally assume that the initial invitation is tantamount to a final appointment.

This assumption is reinforced by the fact that NIH has sometimes included an invitee's name on membership lists released to the news media and other members of the public, after the invitee has agreed to serve, but before the individual's Form HEW 474 has been reviewed from a conflict-of-interest standpoint.

In the opinion of the Study Team, this approach downplays the importance of the review of the Form HEW 474 to an unwarranted extent, so that once an individual receives an initial invitation and accepts, it will be difficult for the appointing official finally to reject the individual on conflict-of-interest grounds, as well as embarrassing for the individual if such a rejection should occur.

V. RECOMMENDATIONS

A. Regarding Invitations to Members:

1. THAT ALL INITIAL INVITATIONS TO SERVE ON ADVISORY COMMITTEES MAKE IT EXPLICIT THAT FINAL APPOINTMENT IS CONTINGENT UPON REVIEW OF THE COMPLETED FORM HEW 474 FOR CONFLICTS OF INTEREST, OR POTENTIAL CONFLICTS OF INTEREST, AND THAT THE NEW MEMBER BE FORMALLY NOTIFIED OF THE APPOINTMENT AFTER THE APPOINTMENT PROCESS (INCLUDING REVIEW OF THE FORM HEW 474) HAS BEEN COMPLETED.

2. THAT THE NAMES OF NEW MEMBERS OF ADVISORY COMMITTEES NOT BE RELEASED TO THE NEWS MEDIA OR OTHER MEMBERS OF THE PUBLIC UNTIL SUCH TIME AS FINAL APPOINTMENT HAS OCCURRED AFTER COMPLETION OF THE FORMS HEW 474.

APPOINTMENT OF EMPLOYEES OF FOR-PROFIT ORGANIZATIONS TO NIH INITIAL REVIEW GROUPS

Under current NIH policy, full-time employees of "for-profit" organizations and institutions may not be appointed to National Institutes of Health Initial Review Groups (IRGs). At the request of the Office of the Director, the Grants Peer Review Study Team was asked to review this policy and to formulate a recommendation either as to its retention or for change.

I. BACKGROUND*

The present NIH policy which does not permit the appointment of individuals employed by profit-making organizations to serve on initial review groups had its origin in two Presidential documents, issued in February 1962 and signed by President John F. Kennedy. The first, dated February 9, 1962, was relevant strictly to "Preventing Conflicts of Interest on the Part of Advisors and Consultants to the Government."(1) The second, dated February 26, 1962, was issued as Executive Order #11007, "Prescribing Regulations for the Formation and Use of Advisory Committees."(2) Both of these documents now have been superseded by more recent issuances, the first in Regulations, Title 45, Part 73, Standards of Conduct(3), promulgated by HEW, and the second by the Federal Advisory Committee Act of 1972 (P.L. 92-463)(4).

The Presidential memorandum of February 9, 1962, set down specific rules for the appointment of consultants and advisors so that a determination could be made as to the status of each individual in regard to conflict of interest. The necessity for such rules was precipitated by an opinion rendered by the then Attorney General concluding that the conflict of interest statutes of the United States Code (18 U.S.C. 281) applied not only to consultants and advisors when they were actually employed by the Government, but were also applicable throughout the entire period of such appointments when employment by the Government was for forty percent or more of the total appointment period. Section 281, 18 U.S.C., in general, precludes a government employee from acting in matters that come before government departments or agencies on behalf of a nongovernment employer from which he or she receives compensation. The effect of this regulation was to require NIH to make a determination about each consultant as to whether or not his non-Federal employment status created a conflict of interest situation while such an individual was acting as a consultant to the Government. The present HEW Form 474 is used for this purpose (Appendix E-2).

Based on this Presidential memorandum, instructions were given to NIH to terminate all committee memberships as of July 30, 1962, and to reappoint only those individuals about whom it could be determined that there was no conflict of interest. It was found that there was considerable difficulty in making such a determination in regard to consultants who were employees of profit-making organizations, and thus a decision was made not to reappoint such persons.

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*A file on all the background and historical documents is available in the Grants Peer Review Study Team office.

The pertinent portions of Executive Order #11007 stated that no advisory committee might be formed unless it was specifically authorized by law or specifically determined as a matter of formal record by the head of a department or agency to be in the public interest. Further, any advisory committee, the duration of which was not fixed by law, was to terminate not later than two years from its date of formation unless the department or agency head specifically determined that its continued existence was in the public interest. However, Section 9 of this Executive Order stated that these requirements did not apply "to any advisory committee composed wholly of representatives of state or local agencies or charitable, religious, educational, civic, social welfare, or other similar nonprofit organizaton." This reinforced the NIH decision to exclude employees from "for-profit" organizations from serving on advisory committees (except on those established by specific legislation, e.g., advisory councils).

As stated previously, Executive Order #11007 was superseded by the Federal Advisory Committee Act of 1972 (P.L. 92-463). However, the section of the Executive Order quoted above was not included as part of the new legislation; and thus all committees established and utilized by NIH, regardless of their composition, are now subject to the requirements of the Federal Advisory Committee Act. Current Federal regulations (Title 45, Part 73), established under this act, are equally applicable to all consultants regardless of the profit status of their non-Federal employers.

Thus there must be adequate review of the Conflict of Interest Statements (HEW Form 474) filed by all consultants; and there no longer seems to be any barrier to the appointment of employees of profit-making organizations to serve on any NIH committee, provided it can be determined that a conflict of interest does not exist when performing the required duties. Indeed, the requirements in this regard must be no different for individuals who are employees of nonprofit organizations.

II. CONSIDERATIONS BY THE GRANTS PEER REVIEW STUDY TEAM

The Study Team recognizes that competent scientists are employed by both nonprofit and "for-profit" organizations. Furthermore, it is evident, upon review of the Conflict of Interest Statements (HEW Form 474) currently on file, that many present appointees to NIH IRGs are consultants to industrial firms and are not considered to have a conflict of interest as far as service on an NIH IRG is concerned. It would thus appear that both consultants to and employees of industry should be considered equally satisfactory for service on NIH initial review groups.

The Study Team also is of the opinion that, in some areas of scientific research, persons with certain talents, skills, and expertise are to be found in the industrial sector who do not exist in adequate numbers in the nonprofit sector. The use of such experts, when needed in the review of grant applications, should not be denied to NIH. Furthermore, in recent years, NIH has had to draw on an ever-increasing number of individuals for consultant services in order to meet certain other requirements; thus, the pool of scientifically knowledgeable individuals has to be increased. The untapped resource of employees of the industrial sector would reinforce the pool of available experts upon which NIH can draw for consultant services.

Based on the above considerations and the background information, the Study Team makes the following recommendations:

III. RECOMMENDATIONS

THAT EMPLOYEES OF "FOR-PROFIT" ORGANIZATIONS BE ELIGIBLE FOR MEMBERSHIP ON ALL INITIAL REVIEW GROUPS CONSIDERING GRANT APPLICATIONS (INCLUDING NATIONAL RESEARCH SERVICE AWARD APPLICATIONS), AND THAT THE SELECTION OF THESE INDIVIDUALS BE BASED ON THE SCIENTIFIC PROFICIENCY NECESSARY TO FILL THE VARIOUS NEEDS OF SPECIFIC COMMITTEES. THE BASIS FOR SELECTION OF SUCH SCIENTISTS SHALL BE THE SAME AS FOR THOSE EMPLOYED BY "NON PROFIT" ORGANIZAITONS.

THAT THE CONFLICT OF INTEREST STATEMENTS (HEW FORM 474) SUBMITTED BY EMPLOYEES OF PROFIT-MAKING ORGANIZATIONS NOMINATED AS CONSULTANTS MUST BE REVIEWED BY THE AGENCY HEAD, WHO MUST BE SATISFIED THAT EACH NOMINEE CAN SERVE ON THE SPECIFIC COMMITTEE FOR WHICH SUCH PERSON HAS BEEN PROPOSED WITHOUT BEING IN VIOLATION OF THE CONFLICT OF INTEREST STATUTES.

IV. CRITERIA FOR SELECTION OF IRG MEMBERS FROM "FOR-PROFIT" ORGANIZATIONS

The Study Team suggests that the criteria stated below be considered as part of the above recommendation:

1. General

THE CRITERIA FOR SELECTION OF IRG MEMBERS FROM "FOR-PROFIT" ORGANIZATIONS SHOULD BE NO DIFFERENT THAN THOSE FOR SELECTION OF PERSONS FROM "NONPROFIT ORGANIZATIONS."

2. Specific

a. Recognized achievement in a relevant field.

For scientific merit review of research grant applications, an individual serving on an NIH initial review group should have particular competence as an independent investigator in the needed scientific discipline or research specialty. Demonstration of such competence is based on the quality of research accomplished, productivity in the form of research reports published in scientific journals, and other significant scientific activities, accomplishments, and honors.

Assessment is based primarily on the general recognition by a scientist's peers of the individual's abilities and accomplishments as a careful, critical, original, and deliberate investigator, and as a broadly based expert in the past development and current advancement of a particular field of research. Usually the M.D., Ph.D., or equivalent advanced degree is required; however, a doctoral degree is not required in those circumstances where an individual's experience clearly indicates outstanding competence.

Furthermore, a reviewer of applications for National Research Service Awards must have had substantial experience in graduate research training, with an active interest in the methods and planning of research training in his or her discipline, field, or specialty, and a record of accomplishment in such training.

b. Mature judgment and objectivity as a scientist.

The characteristics of mature judgment, balanced perspective and objectivity are essential to the merit review process.

c. Ability and willingness to serve.

(a) A candidate must have demonstrated the capability of working effectively in a group context.

(b) A candidate who accepts appointment to an NIH advisory group is expected to accept responsibility for all work assignments and must protect the confidentiality of applications and reviewer opinions.

d. Conflict of interest.

The Conflict of Interest Statement (HEW Form 474) submitted by a nominee must be reviewed by the agency head who must be satisfied that the individual can perform as a special government employee without being in violation of conflict of interest statutes.

V. Implementation of the Study Team Recommendation

In order to implement the above recommendation, guidelines for the selection of all consultants to NIH initial review groups, but particularly those in regard to persons from "for-profit" organizations, should be reviewed periodically; and, where necessary, modifications should be made so that the criteria for selection of individuals are such as to retain the quality of consultant advice that NIH expects from its advisors.

APPOINTMENT OF EMPLOYEES OF FOR-PROFIT ORGANIZATIONS TO NIH INITIAL REVIEW GROUPS

References

1. Kennedy, John F., Preventing Conflicts of Interest on the Part of Advisors and Consultants to the Government; February 9, 1962.

2. Kennedy, John F., Prescribing Regulations for the Formation and Use of Advisory Committees. Executive Order #11007, February 26, 1962.

3. Regulations, Form HEW-539, Title 45, Part 73, Standards of Conduct, September, 1970.

4. Public Law 92-463, Federal Advisory Committee Act, October 6, 1972.

LEGAL CONSIDERATIONS REGARDING GRANTS PEER REVIEW

LEGAL CONSIDERATIONS REGARDING GRANTS PEER REVIEW

In recent years, the public, the Congress, the Executive Branch, and the courts have become increasingly attentive to and supportive of greater public accountability and openness in Government. Recent legislation, court cases, and proposed legislative action in this area have important implications for the operation and continued effectiveness of the NIH peer review system. At the same time, the Congress and the President's Biomedical Research Panel have strongly endorsed this system, praising its fairness, objectivity, and high quality. The strengths of the system must be preserved while it is kept publicly accountable.

I. Background

A. Peer Review Legislation

In discussing the peer review system in a 1973 report, the Senate Committee on Labor and Public Welfare endorsed the comment that: "The peer review system [at NIH] has given us the best science through a federal agency with the least political interference of any governmental process ever developed. It is truly one of the great achievements of American government. . . " Under this system, each application to NIH for a research grant is initially reviewed for scientific merit by a group of experts in relevant biomedical fields. The recommendation of the group is then considered by an appropriate National Advisory Council or Board, made up of both scientists and lay persons, which makes a final recommendation to NIH on the application based not only on scientific merit but also program priority.

Each of the NIH Bureaus, Institutes, and Divisions (BIDs) awarding grants has a National Advisory Council or Board whose members generally are leaders in science, education, or public affairs. Section 301 of the Public Health Service (PHS) Act [42 U.S.C. 241], the basic authority under which NIH research project grants are made, provides that no grant may be awarded for a research project unless it is first "recommended" for funding by the appropriate National Advisory Council or Board. 1/ A similar requirement exists in Section 472 of the PHS Act [42 U.S.C. 289l-1], relating to National Research Service Awards to individuals for research and research training. In addition, a number of the Councils are assigned responsibilities for advising their respective BIDs on other specific program activities (Title IV, PHS Act, 42. U.S.C. 281 *et seq*.).

1/ By statute, the National Cancer Institute and the National Heart, Lung, and Blood Institute may award research project grants, without Council or Board review and recommendation, where the awards are for $35.000 or less in direct costs.

In general, the National Advisory Councils were established by law at the same time as the BIDs to which they give advice and have been responsible, by statute, for advising on grant applications since their establishment. On the other hand, prior to the passage of the National Cancer Act of 1971 [Public Law 92-218], there was no actual statutory requirement that research grant applications be reviewed as well by scientific peer review groups, e.g., NIH Initial Review Groups (IRGs). However, the National Cancer Act of 1971 amended the PHS Act to require the Director, NCI, to ". . . provide for proper scientific review of all research grants . . . over which he has authority (1) by utilizing to the maximum extent possible, appropriate peer review groups established within the National Institutes of Health . . . and (2) when appropriate, by establishing . . . other formal peer review groups as may be required." [42 U.S.C. 2863.]

The National Cancer Act Amendments of 1972 [Public Law 93-352] in effect extended this requirement of scientific peer review to all " . . . applications . . . for grants under . . . [the PHS] Act for biomedical and behavioral research . . . " (Appendix F-1). It further required that such review be conducted " . . . to the extent practical . . .. in a manner consistent with the system. . . " being utilized on the date the amendments became effective and that not more than one-quarter of the members of any peer review group may be officers or employees of the United States, presumably apart from their service as members of the group. [42 U.S.C. 289l-4.]

On March 29, 1976, a notice of proposed rulemaking (Appendix F-2) was published in the Federal Register setting forth proposed regulations to implement the foregoing provisions (41 F.R. 12986). In general, these regulations codify the pre-existing NIH system for scientific peer review of grants.

The Health Research and Health Services Amendments of 1976 [Public Law 94-278] include a section which states in pertinent part that "All appointments to advisory committees established to assist in implementing the Public Health Service Act . . . shall be made without regard to political affiliation."

B. Conflict of Interest

With regard to conflict of interest, the aforesaid regulations would essentially codify prior NIH practice which requires that members of peer review groups in no way participate in deliberations or actions concerning applications from their own institutions and that they leave the room before any such discussions. Where one of the members of the group, which would normally review a particular application, is actually named in the application as the principal investigator or a principal staff member, the application is assigned to another peer review group with the requisite competence, or if none exists, it is assigned to a special (ad hoc) initial review group, no more than 50 percent of the membership of which may be from the groups of which the investigator or staffer is a member.

These actions have been considered sufficient by the Department of Health, Education, and Welfare to avoid any violation of the conflict-of-interest statutes, and they were endorsed by the Congressional committees which considered the proposals concerning peer review that were finally enacted as part of Public Law 93-352.

The foregoing conflict-of-interest requirements were recently upheld by the United States District Court for the Northern District of California, in an opinion dated February 10, 1976, stating that these requirements " . . . designed to avoid conflicts of interest in the grant application review process [are] adequate and reasonable . . . " (Grassetti v. Weinberger, et al., C.A. No. C-75-1198-SC.) (Appendix F-3).

II. The "Sunshine" Laws

The so-called "Sunshine" Laws affecting NIH are the Freedom of Information Act (FOIA), the Privacy Act, the Federal Advisory Committee Act, and the Government in the Sunshine Act.

The general thrust of the Freedom of Information Act is to make available to the public upon request any and all of the records of Federal agencies. The Privacy Act seeks to protect the individual from the Government's collection and use of data concerning the individual without his or her knowledge or consent and to provide the individual access to those Government records which include information pertaining to him or her. The Federal Advisory Committee Act seeks to assure public knowledge about meetings of Governmental advisory groups as well as public access to these meetings. The Government in the Sunshine Act sets forth the circumstances in which NIH may close advisory group meetings to the public.

The specific provisions of the above Acts impact on the peer review system because of their applicability to all significant documents connected with the process and to the operation of committees charged with peer review. The current and potential effects of these laws and proposed amendments to them are examined below in terms of changes in the peer review system, the quality of the peer review process, and related consequences.

A. Freedom of Information Act

1. Description of the Law.

This Act (FOIA), originally adopted in 1967 [Public Law 90-23] and amended in 1974 [Public Law 93-502], requires agencies to make available to the public for inspection and copying, any requested Government records. (Appendix F-4). However, subject to provisions of the Privacy Act, discussed below, requests for records may be denied if those sought fall within any one of nine specified "exemptions" set forth in the FOIA. Only the following three exemptions presently bear on the peer review system:

> "(4) trade secrets and commercial or financial information obtained from a person and privileged or confidential;
>
> "(5) inter-agency or intra-agency memorandums or letters which would not be available by law to a party other than an agency in litigation with the agency;
>
> "(6) personnel and medical files and similar files the disclosure of which would constitute a clearly unwarranted invasion of personal privacy."

2. The Washington Research Project Case.

Until recently, all requests under the FOIA for grant applications (funded or unfunded), interim progress reports, summary statements of IRG reviews, and site visit reports were denied, based primarily on exemption 4. To the extent that these materials contained discussions of the qualifications of individuals, such discussions were considered to be protected as well by exemption 6. Additionally, since the summary statements and site visit reports reflected the deliberations and opinions of the members of advisory groups and site visitors, these items were also viewed as being within exemption 5.

In Washington Research Project, Inc., v. Weinberger, et al., a public interest group challenged the Department's denial of the plaintiff's request for the foregoing categories of documents, relating to specific funded initial applications and funded and unfunded continuation, supplemental, and renewal applications. The United States District Court for the District of Columbia ruled that essentially all the materials in question were available under the FOIA. (366 F. Supp. 929.) On appeal, the Court of Appeals for the District of Columbia agreed with the lower court that the protocols and progress reports at issue were not covered by exemption 4, but held that the portions of summary statements and site visit reports reflecting the deliberations of members of advisory groups and site visitors were exempt from mandatory disclosure under exemption 5. (504 F. 2d 238.) (Appendix F-5). In light of the appeals court decision, which is now final, current Department practice is to release, upon request, funded initial grant applications and continuation, supplemental, and renewal applications (whether or not funded), as well as interim progress reports, except insofar as disclosure of any particular items would adversely affect patent or other valuable rights. In so doing, any confidential financial information concerning the applicant or grantee will be deleted. As in the past, requests for summary statements and site visit reports will largely be denied.

3. Availablity of applications.

It is almost impossible in the short run to measure the impact of the FOIA and the Washington Research Project decision on the peer review process. It has been suggested, however, that scientists may be less willing to include new ideas in their applications if they may be available to the public, thereby diminishing the usefulness of such an application in determining which projects to fund. If applicants do react in this way, reviewers will have to depend more heavily on site visits, the general reputation of investigators, and subjective evaluations. This will tend to work against the new investigator and the novel idea.

In its Report to the President and Congress, dated April 30, 1976, the President's Biomedical Research Panel, established pursuant to Public Law 93-352, concluded that this was a real concern and recommended that:

> "The Public Health Service Act . . . should be amended to provide a statutory exemption from disclosure [under] . . . the Freedom of Information Act for research designs and protocols contained in grant applications . . . until the grant . . . funds have been received by the grantee institution . . . Unfunded grant applications . . . should remain confidential."

In a second Report (June 30, 1976) mandated under the Health Research and Health Services Amendments of 1976 [Public Law 94-278] and entitled "Disclosure of Research Information," the Panel reaffirmed its prior conclusion, expressing the conviction that unless the intellectual property rights of investigators are adequately protected, ". . . the federal biomedical and behavioral research effort and its impact on the health of the nation—is likely to be impaired . . . " The Panel went on to say that disclosure of information from research protocols did not appear to contribute to the improvement of peer review. Rather, the Panel expressed the belief that such disclosure ". . . could impair the ability of the [peer review] system to ensure high-quality federally funded research." It pointed out that ". . . the credibility of peer review would certainly be undermined if it were compromised by the submission of derivative proposals and applications and if the judgments by peer review groups were based on incomplete information."

The findings set forth in the Survey of the NIH Research Grant Peer Review System, reported elsewhere herein, tend to support the conclusions of the Panel, for only one-eighth of the advisory committee members surveyed favored the public release of all renewal and continuation applications whether or not they had been funded. (Supplement A, p. 55)

RECOMMENDATION:

THAT THE PHS ACT BE AMENDED TO PROVIDE STATUTORY EXEMPTION FROM THE REQUIREMENTS OF THE FREEDOM OF INFORMATION ACT FOR DISCLOSURE OF RESEARCH DESIGNS AND PROTOCOLS PRESENTED IN GRANT APPLICATIONS.

(This is, in essence, an endorsement of the recommendation of the President's Biomedical Research Panel.)

B. Federal Advisory Committee Act (FACA)

1. Description of the Law.

This Act (FACA), approved on October 6, 1972 [Public Law 94-463], states that "No advisory committee shall meet or take any action until an advisory committee charter has been filed with . . . the head of the agency to whom . . . [the] advisory committee reports . . . " (Appendix F-6) The term "advisory committee" is defined (subject to certain exemptions not here relevant) to include " . . . any committee, board, commission, council, conference, panel, task force, or other similar group, or any subcommittee or other subgroup thereof . . . except that such term excludes . . . any committee which is composed wholly of full-time officers or employees of the Federal Government." All meetings of advisory committees must be advertised in the Federal Register, and (subject to the discussion which follows) all portions must be open to the public. Effective until March 1977, there is an exception to the open meeting requirement, where " . . . the head of the agency . . . determines [the meeting] is concerned with matters listed in Section 552(b) of Title 5, United States Code." Section 552 is commonly referred to as the "Freedom of Information Act," and paragraph 552(b) thereof sets forth the exemptions from mandatory disclosure which were discussed above in connection with the FOIA. Thus, until March 1977, a meeting may, in effect, be closed insofar as the subject matter being discussed, if reduced to writing, would be exempt from mandatory disclsoure under the FOIA.

The Government in the Sunshine Act [Public Law 94-409] recently amended the FACA (effective in March 1977) to permit an exception for a portion of a meeting only where " . . . the head of the agency . . . determines that such portion . . . may be closed in accordance with Subsection (c) of Section 552(b) of Title 5, United States Code." Section 552(b) was added to the United States Code by Public Law 94-409 and deals primarily with the decision-making processes of agencies headed by collegial bodies composed of two or more persons appointed by the President with the advice and consent of the Senate. Subsection (c) of Section 552(b) contains a list of exemptions from the requirement that these bodies hold their meetings in open session. The subsection provides in pertinent part that:

> "Except in a case where the agency finds that the public interest requires otherwise . . . [portions of an agency meeting may be closed] where the agency properly determines that such portion or portions of its meeting . . . is likely to—
>
> * * * * *
>
> "(4) disclose trade secrets and commercial or financial information obtained from a person and privileged or confidential;
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> * * * * *
>
> "(6) disclose information of a personal nature where disclosure would constitute a clearly unwarranted invasion of personal privacy;
>
> * * * * *
>
> "(9) disclose information the premature disclosure of which which would—
>
> * * * * *
>
> "(B) in the case of any agency, be likely to significantly frustrate implementation of a proposed agency action, except that subparagraph (B) shall not apply in any instance where the agency has already disclosed to the public the content or nature of its proposed action, or where the agency is required by law to make such disclosure on its own initiative prior to taking final agency action on such proposal;"

While exemptions 4 and 6 in subsection (c) of Section 552(b) are similar to those in the FOIA, the new law does not contain any exact counterpart to FOIA exemption 5. On the other hand, it does contain a new exemption 9B which could be applicable to meetings of peer review groups.

In considering whether to use an exemption to close a portion of a meeting under subsection (c), an agency head must decide if it is "likely" that exempt matters or information will come up, not simply that there is a possibility of it occurring.

2. What constitutes a "committee"?

A threshold question concerns what constitutes a "committee" for purposes of the FACA. In order to comply with conflict-of-interest requirements or to review certain types of applications, it is regularly necessary to call together what are commonly referred to as "special," or "ad hoc," initial review groups to consider one, or a small number of applications, usually on a one-time basis. The FACA itself does not distinguish between short-term and more permanent advisory groups nor does it make any specific mention of "special," or "ad hoc," groups. In Food Chemical News, Inc., v. Davis, 378 F. Supp. 1048 (D.D.C. 1974), a United States District Court held that two separate informal meetings between Treasury Department officials and consumer and industry representatives, at which the officials secured advice relative to regulations on labeling of distilled spirits, were covered by the FACA. On the other hand, in Nader v. Baroody, 396 F. Supp. 1231 (D.D.C. 1975), another judge in the same court ruled that bi-weekly White House meetings with selected groups from the private sector were not within FACA's ambit. In the latter case, the court cited with apparent approval the following criteria for determining whether a gathering is an "advisory committee" under FACA:

> "(a) Fixed membership, usually selected by a Federal official or determined on the basis of Federal Law;
>
> "(b) Established by a Federal official or on the basis of Federal law; or, if not federally established, the initiative for its use as an advisory body for the Federal Government came from a Federal official rather than from a private group;
>
> "(c) A defined purpose of providing advice regarding a particular subject or particular subjects;
>
> "(d) An organization structure (e.g., officers) and staff;
>
> "(e) Regular or periodic meetings."

Under these criteria, a strong argument could be made that "special," or "ad hoc," groups formed to review a single application or groups of applications are not committees for purposes of FACA.

On February 6, 1976, Senate Bill No. S. 2947 was introduced which, among other things, would amend the definition of "advisory committee" to encompass "ad hoc" groups. (Appendix F-7). In light of the realities regarding time needed to charter committees, passage of this amendment would probably make it impossible to utilize "special" or "ad hoc" groups in the conflict-of-interest situations discussed above, or to meet short term workload increases in specialized areas.

RECOMMENDATION:

THAT NIH AND HEW PRESENT SPECIFIC WRITTEN OPPOSITION TO ANY LEGISLATIVE PROPOSAL EXTENDING THE FEDERAL ADVISORY COMMITTEE ACT TO "AD HOC" OR SPECIAL REVIEW GROUPS, BASED ON THE NEED TO PREVENT CONFLICTS OF INTEREST IN THE PEER REVIEW OF GRANT APPLICATIONS AND THE NEED TO RETAIN FLEXIBILITY IN MEETING SHORT TIME WORKLOAD INCREASES IN SPECIALIZED AREAS.

3. Closing meetings.

When the FACA was first passed, portions of meetings devoted to review of grant applications were closed primarily under FOIA exemptions 4 and 6, on the basis that discussions were likely to cover matters affecting patent and other valuable rights, and that negative comments about the qualifications and ideas of investigators and others could institute a clearly unwarranted invasion of their personal privacy. While the deliberations of the peer review groups would seem also to be covered by exemption 5, the Department severely limited use of this exemption, to avoid excessive closure of meetings. In the Washington Research Project case, however, the Court of Appeals indicated that the deliberations of the peer review groups, as set forth in the summary statements, deserved exemption 5 protection. As a result, the Department agreed to a relaxation of its restrictions on using exemption 5 to close meetings. Since then, exemptions 4, 5, and 6 have all been cited as bases for closure to review grant applications.

On April 6, 1976, the United States Court of Appeals for the District of Columbia, in Aviation Consumer Action Project, et al., v. Washburn, et al., ruled that exemption 5 was available as a basis for closing of meetings, thereby overturning, in effect, several lower court opinions to the contrary. This decision provided added support for use of that exemption in connection with review of grant applications (Appendix F-8).

As has already been indicated, Public Law 94-409 carried forward exemptions 4 and 6, as grounds for closing meetings, essentially without change. Thus, where applicable, these exemptions can continue to be cited to support the closure of meetings. On the other hand, in its Report on Public Law 94-409, the Conference Committee indicated that one of the purposes of the amendment to FACA was to overrule the Washburn decision. At the same time the conferees stated that:

> "The conferees, however, are concerned about the possible effect of this amendment upon the peer review and clinical trial preliminary data review systems of the National Institutes of Health. The conferees thus wish to state

> as clearly as possible that personal data, such as individual medical information, is especially sensitive and should be given appropriate protection to prevent clearly unwarranted invasions of individual privacy. While the conferees are sympathetic to the concerns expressed by NIH regarding its committees' funding recommendations and analysis of preliminary data, the conferees are equally sympathetic to concerns expressed by citizens' groups that important fiscal and health-related information not be unnecessarily withheld from the public.
>
> "With these competing interests in mind, the conferees have secured assurances that the appropriate House and Senate committees will review the unique problems of NIH under the new standards. Indeed, it is noted that the subcommittee on Reports, Accounting and Management of the Senate Government Operations Committee has already held three days of hearings on this matter and plans to continue with further inquiry at an early date."

In its Report of April 30, 1976, the President's Biomedical Research Panel noted the important concern expressed in evidence to the Panel over the possibility that meetings of peer review groups might be opened to the general public. The Panel noted that:

> "Aside from the obvious likelihood of a decrease in the candor of the reviewers and the lack of any indication of how the members would be protected professionally and financially from vengeful acts and charges of libel or bribery, open meetings would have at least two other objectionable features. The first would be the reluctance of many qualified scientists to participate as members of Study Sections and Review Committees. The second concern relates to the untenable discrimination between investigators from two classes of applicant organizations: those able to send observers because of geographic proximity to the review meeting, local representation, or financial means, and those unable to take advantage of the open sessions."

The Panel therefore recommended that the Public Health Service Act be amended ". . . to provide statutory assurance that the initial review for scientific and technical merit ("peer review") remain totally confidential."

This is in accord with the overwhelming sentiment of members of advisory groups surveyed by the NIH Grants Peer Review Study Team. Only five percent were in favor of opening IRG sessions to the public, and just seven percent supported such opening simply to the investigator. Figures were only slightly higher for Advisory Board and Council sessions concerned with grant application review. (Supplement A, p. 56).

C. Privacy Act (PA)

1. Description of the Law.

This Act, approved on December 31, 1974, and effective in pertinent part on September 27, 1975, [Public Law 93-579], regulates the collection, maintenance, and use of information about individuals in agency systems of records. (Appendix F-9) The term "system of records" is defined to cover " . . . a group of any records under the control of any agency from which information is retrieved by the name of the individual or by some . . . other identifying particular assigned to the individual." The Privacy Act also requires agencies to establish procedures under which such individuals are given access to and an opportunity to seek correction or amendment of this information.

2. Availability of summary statements and reviewers' opinions to the principal investigator.

On March 8, 1976, the Associate Director for Extramural Research and Training, NIH, published an Instruction and Information (I&I) Memorandum, OERT 76-1, concerned primarily with the Privacy Act, but dealing as well with the FOIA. (Appendix F-10). The I&I memorandum indicates that: "All NIH official files for grants . . . " constitute a system of records under the Privacy Act. In discussing release of information under the Privacy Act to such individuals as principal investigators, program directors, and fellowship awardees, the I&I memorandum further provides that:

> "When the subject individual makes a request for information from a record pertaining to him/her, all fact and opinion pertaining to the subject individual of the record should be released to him/her under the PA. (The names of the outside reviewers, i.e., persons who are not members of standing or _ad hoc_ committees and who, at the request of NIH staff, submit opinions in writing, are not to be listed on the summary statement, but are listed elsewhere in the official record). In addition, all _factual_ information from the summary statement is available to the subject individual, e.g., name of investigator, title of project, description of project, budget of project."

The I&I memorandum goes on to state that the priority score will not be released and the NIH " . . . will release verbatim only what is required under both PA and FOIA." [3]

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[3] The priority score is not considered by NIH to be "about" the investigator and is therefore not subject to the Privacy Act. Also, NIH takes the position that release of the priority score is not required under FOIA because it falls within exemption 5.

With respect to site visit reports and opinions of outside reviewers (persons whose opinions have been obtained but who are not members of the review group), the I&I memorandum states:

> "NIH has generally taken salient information from the site visit reports and outside opinions for incorporaton into the summary statement. After information is incorporated into the summary statement, the site visit report and outside opinions are not retained.
>
> "If a specific request [by the principal investigator] is made for access to these reports while still in our [NIH's] possession, appropriate information . . . from them must be provided to the requester.
>
> "The name of an outside reviewer is not deleted from documents representing his/her opinions, e.g., mail reviews, site visit notes submitted by individual site visitors. When Executive Secretaries prepare site visit reports and summary statements, they will not ordinarily associate individual opinions with reviewers names, however."

The survey of advisory group members conducted by the Study Team showed that 79 percent of those responding favored the rule set forth in the I&I memorandum of releasing the summary statement to the principal investigator without the priority score. (Supplement A, Page 56.) However, a substantial majority, 60 percent, believed that the priority score should also be released, with an additional seven percent indicating that release would have no effect on the quality or efficiency of the peer review system. A majority, 53 percent, supported release of individual reviewers' opinions if the reviewers remained anonymous, with an added six percent in the "no effect" category, but only five percent would agree to such disclosures with the reviewers identified.

In the judgment of the Study Team, these results reflect a recognition on the part of those surveyed that the principal investigator has a right to the complete and detailed record of review group action on his or her research proposal. This is evidenced by the respondents' overwhelming support for release of the summary statement, with or without the priority score, to the investigator. On the other hand, if an individual reviewer is identified to the investigator as having expressed a particular opinion on the proposal, whether or not adopted by the review group as a whole, this is likely to affect the candor of such opinions in the future. As a result, those surveyed did not favor release of such opinions unless the reviewers' anonymity was protected.

The Study Team believes that, inasmuch as the review group members themselves do not believe that summary statements should be kept from the principal investigator, there is little justification for not so releasing them, whether or not required to do so under the Privacy Act. Also, for reasons outlined elsewhere in this report, the Study Team feels that the priority score should not be deleted.

With respect to individual reviewers' opinions, the Study Team recognizes that, in accordance with the Privacy Act, these must normally be released to the principal investigator. In view of the survey results indicating that more than half of those responding were in favor of such disclosure, provided that the reviewers' identity is not revealed, the Study Team sees no substantial reason for objecting thereto. At the same time, the Study Team urges that steps be taken to establish an adequate legal basis for protecting the reviewer's anonymity either through reinterpretation of existing law, if reasonably possible, or enactment of new legislation, under circumstances in which release of reviewers' opinions is required under the Privacy Act.

RECOMMENDATIONS:

THAT THE SUMMARY STATEMENTS BE MADE AVAILABLE UPON REQUEST TO THE PRINCIPAL INVESTIGATORS, INCLUDING THE PRIORITY SCORES.

THAT IN RELEASING REVIEWERS' OPINIONS UNDER THE PRIVACY ACT, AN ADEQUATE LEGAL BASIS BE ESTABLISHED FOR PROTECTING THE REVIEWERS' ANONYMITY EITHER THROUGH REINTERPRETATION OF EXISTING LAW, IF REASONABLY POSSIBLE, OR ENACTMENT OF NEW LEGISLATION.

D. Effect of Sunshine Laws on Scientific Progress

It is clearly not possible at the present time to make an assessment of the effect of the recent Sunshine Laws on the progress of biomedical research in this country. However, it seems possible that, if investigators conclude that it is contrary to their best interests to include details of new experimental and theoretical approaches in research proposals, the vagueness of such applications will be interpreted as weakness in the scientific undertaking. This could result in the stifling of innovative research and decreased support of new investigators who have little or no background or history of research activity upon which reviewers may judge merit. If so, the very life blood of scientific progress, namely innovation, and youth may be lost.

Thus, it would appear essential that the impact of this new legislation be evaluated periodically so as to assess these factors.

The Study Team believes that the concerns expressed by the President's Biomedical Research Panel are well founded. If NIH were required to open meetings, it is likely that reviewers would have reservations about providing candid, negative criticisms of research proposals. This would undoubtedly result in ambiguous and superficial evaluations, reducing the quality of the judgments by the review groups and thus the quality of NIH granting operations, which allocated about $1.3 billion in fiscal year 1975. Also, some individuals might even be unwilling to serve on review groups for fear of harassment.

RECOMMENDATION:

THAT THOSE PORTIONS OF THE MEETINGS OF ADVISORY GROUPS WHICH INVOLVE THE REVIEW OF GRANT APPLICATIONS CONTINUE TO BE CLOSED TO THE PUBLIC (INCLUDING THOSE SUBMITTING APPLICATIONS), EITHER UNDER CURRENT EXEMPTIONS TO THE OPEN-MEETING REQUIREMENT OR THROUGH A STATUTORY AMENDMENT

4. Review Group Membership

Another change in the FACA proposed in Senate Bill No. S. 2947 (referred to above) would be to require that one-third of the members of each Federal advisory group " . . . be drawn from citizens in private life who shall represent the interests of the Public . . . " (Appendix F-7). If such a provision is interpreted to require the appointment of individuals without scientific expertise to IRGs, this would destroy the fundamental principle of the dual peer review system, namely that the quality of research proposals is best assessed by experts in particular fields, who are scientific or technical "peers" of the applicants. This is accomplished at the first stage of review by the IRGs. Broader public input is then obtained at the second level of the dual review system when the proposals are considered by the National Advisory Councils and Boards made up of leaders, not only in scientific fields, but also in education and public affairs.

RECOMMENDATION:

THAT, NOTWITHSTANDING PROPOSALS WHICH WOULD REQUIRE PUBLIC REPRESENTATION ON ALL COMMITTEES, THE CURRENT SYSTEM OF DUAL REVIEW BE PRESERVED, WITH GRANT APPLICATIONS BEING REVIEWED FIRST BY INITIAL REVIEW GROUPS CONSISTING SOLELY OF SCIENTIFIC AND TECHNICAL EXPERTS AND THEN BY NATIONAL ADVISORY COUNCILS AND BOARDS WHICH INCLUDE REPRESENTATIVES OF THE PUBLIC.

RECOMMENDATION:

THAT THE NIH AND HEW ESTABLISH A MECHANISM FOR SPECIAL, PERIODIC ASSESSMENT OF THE IMPACT OF THIS NEW LEGISLATION ON THE QUALITY OF GRANT APPLICATIONS AND ON THE QUALITY OF PEER REVIEW OF SUCH APPLICATIONS AND THAT SUCH ASSESSMENT BE REPORTED TO THE LEGISLATIVE AND EXECUTIVE BRANCHES OF THE GOVERNMENT.

IMPACT OF REVIEW WORKLOAD ON QUALITY OF INITIAL MERIT REVIEW

IMPACT OF REVIEW WORKLOAD ON QUALITY OF INITIAL MERIT REVIEW

Over the past several years the workloads of many of the Initial Review Groups (Study Sections) have increased to levels which can only be defined as excessive. During a period in which there were reductions in the federal workforce, including the NIH peer review workforce, and new controls were placed on the formation and use of advisory groups, the peer review workload increased in size and complexity. The demands being placed upon already overburdened peer review group members and NIH executive secretaries will, unless corrective actions are taken very soon, result in a lower quality of peer assessments and recommendations.

I. BACKGROUND

Members of NIH grant review groups have expressed a decided reluctance to assume further peer review responsibilities: 97% of study section members, 86% of members of other NIH initial review groups, and 90% of responding members of Advisory Councils and Boards rated the suggestion that initial review group members' workload be increased as poor or very poor, in the survey conducted by the Study Team (Supplement A). In addition, there are currently problems in regard to (1) the adequacy of summary statements, (2) loss of continuity and quality in communications within the NIH on peer review issues, and (3) inadequate flow of information between NIH staff and applicant-investigators. The administration of the NIH peer review process, including quality control, is becoming more difficult, as the workload increases quantitatively and also becomes more complex.

THE STUDY TEAM IS CONVINCED THAT WORKLOAD PROBLEMS MUST BE RESOLVED NOW, TO PREVENT DETERIORATION OF THE SYSTEM. IN PRESENTING RECOMMENDATIONS WHICH ADDRESS THIS PROBLEM, THE STUDY TEAM EMPHASIZES THE IMPORTANCE OF MAINTAINING THE INTEGRITY OF THE PEER REVIEW SYSTEM AND ITS ABILITY TO FUNCTION EFFECTIVELY.

The international respect accorded the NIH and the major role of the NIH in biomedical research over the years, have been achieved, in large part, through the accomplishments of NIH grantees, whose research was supported by about 60% of the total NIH budget in fiscal year 1975. Since the NIH peer review system is the primary quality control mechanism for the allocation of grant funds, the quality and consistency of the grant peer review process must receive full respect and protection from any forces which threaten its integrity.

The Study Team is also sensitive to the fact the "peers" must be considered a resource to be protected. The NIH should not abuse or alienate this resource, which has performed so well over the years.

II. CURRENT STATUS OF THE NIH GRANTS PEER REVIEW SYSTEM, IN TERMS OF IRG WORKLOAD

The workload problem is the first issue to be raised whenever NIH staff members discuss the peer review system. DRG has sent forward to OD, NIH, several reports of studies by executive secretaries, suggesting alternate ways to reduce Study Section workloads. Participants in workshops sponsored by the NIH Executive Secretaries Review Activities Committee (ESRAC), reported in recent recomendations to the Deputy Director, NIH,(1) calling attention to the seriousness of this problem.

The Study Team is concerned that review overload either already has or will have adverse effects on:

- The quality of peer review recommendations.
- The ability of NIH to recruit and retain outstanding experts to serve on peer review groups.
- The willingness of peer group members to take on more work.
- The openness of peer review groups to unique or "unorthodox" research ideas.
- The ability of NIH to recruit and retain individuals of high competence to serve as peer review group executive secretaries.
- The quality of documentation of peer review recommendations and technical critiques in site visit reports and summary statements.
- The ability of NIH staff responsible for peer review to perform to their best capability.
- The quality and extent of the assistance that executive secretaries can provide applicants prior to review by identifying additional information needed.
- The quality and depth of information provided to grant applicants on the substance of the peer review recommendations and specific critiques.
- The timely review of grant applications.

The increased workload of grant review groups is most readily demonstrated in terms of the number of applications per review group per review session, shown in Figures 1, 2, and 3. This increase has been accompanied by a decline in the review workforce. The number of grant applications reviewed by individual DRG Study Sections in May and June 1976 for the September/October 1976 Advisory Council meetings ranged from 30 to 128. The 50 Study Sections which met at that time averaged 94 applications per meeting. Twenty-four reviewed over 90 applications each, excluding the review, on an emergency basis, of large numbers of fellowship applications either at that meeting or at a special additional meeting scheduled a few weeks earlier.

Study Section workloads of 90 or more applications require that the meeting last up to four days, often going into evening sessions. Reviewers are exhausted by the end of such marathon meetings. The ability of even the most highly skilled executive secretaries to capture the substance of the review group discusson of over 90 applications at a session, accurately and in detail, must be questioned.

Executive secretaries who are best qualified to judge the effectiveness of review group performance recommend that the number of applications be adjusted to limit review sessions to no more than two and one-half days each. The Study Team regards this as an appropriate guideline in establishing workload ceilings for review groups.

Furthermore, the unremunerated effort of each review group member, in preparation for the review meetings, averages some three work weeks, or 120 hours, of detailed study and preparation of reports. The quality of a reviewer's preparation for meetings with heavy loads is visibly less complete than for meetings at which the review load per individual is appropriate to the effort (done on a reviewer's "own-time") required for careful study before the meeting.

It should be emphasized that the number of applications per review group per review session is only a partial indicator of workload and far from the definitive one. The review process also has become more complex and the following factors must be considered:

- As the increase in the NIH budget has tapered off, and competition has increased for limited grant funds, the number of reapplications also has increased.
- Research has grown more complex: the NIH receives more multi-project applications involving complex technology which are more difficult and time-consuming to review.
- Related to the above, research budget proposals must receive more scrutiny.
- Increased pressure from applicants for detailed reports of the review increases the time required to review each applicaton.
- Review procedures are more rigorous now and documentation is required in regard to the use and protection of human subjects, the appropriate use and care of research animals, the avoidance or containment of hazards to human life and health, and the environment associated with biomedical research.

- The requirements of the "sunshine laws," discussed in another section of this report, have added to the complexity.

III. RECOMMENDATIONS AND DISCUSSION

1. THAT THE DIRECTOR, NIH, TAKE IMMEDIATE STEPS TO LIMIT THE WORKLOAD OF ALL INITIAL GRANT REVIEW GROUPS TO A LEVEL WHICH IS COMPATIBLE WITH MAINTAINING THE HIGH QUALITY OF PEER REVIEW.

   The excessive review workloads imposed on a number of DRG Study Sections and some special review groups in the awarding Institutes and Divisions of NIH must be adjusted downward. Figure 1 illustrates the workload trend for DRG. The remaining recommendations address the implementation of Recommendation 1.

2. THAT AUTHORITY TO ESTABLISH OR DISCONTINUE INITIAL GRANT REVIEW GROUPS, AS THE PEER REVIEW WORKLOAD DICTATES, SHOULD BE DELEGATED TO THE DIRECTOR, NIH.

   Congress has given the Director of the National Cancer Institute authority to establish advisory groups(2). This authority should also be provided to the Director, NIH, to enable the NIH as a whole to respond most effectively to increasing peer review loads and changes in the review workload within scintific and medical fields.

   Although trends do become apparent over time, it is not possible from one grant application receipt date to the next to predict with any accuracy which program areas will stimulate large numbers of applications. The BIDs announce program opportunities through Requests for Applications (RFAs) at irregular intervals and scientific events also stimulate new research efforts in previously dormant disciplines or fields. A small number of special applications at each review round cannot be assigned to a standing review group, either because of the unusual nature of the proposed project or activity, or because the application is submitted by a review group member and must be specially reviewed to avoid a conflict of interest situation. For these applications, *ad hoc* review groups must often be used. If the Director, NIH, *could* establish and dissolve review groups, as recommended, use of review groups for the short-term would be more feasible.

3. THAT ADDITIONAL RESOURCES BE PROVIDED FOR PEER REVIEW OF GRANT APPLICATIONS WHERE ACCEPTABLE ALTERNATIVE APPROACHES TO REDUCTION OF WORKLOAD WILL NOT PERMANENTLY AND EFFECTIVELY RESOLVE LONG-STANDING REVIEW OVERLOADS.

   Where the pool of potential review group members is adequate, existing review groups which are chronically overloaded should be "split" to form two or more groups as needed. *This should be done at once*.

The Study Team recognized that the organization required for peer review procedures cannot be expanded indefinitely. However, no alternatives could be identified which, at present, could be substituted for the additional staff support needed to maintain the quality of review.

4. THAT THE DIRECTOR, NIH, ESTABLISH A PERMANENT MECHANISM TO DETERMINE AND ENSURE AN APPROPRIATE CEILING OR MAXIMUM WORKLOAD FOR EACH NIH INITIAL GRANT REVIEW GROUP.

As the upward trend in average number of applications per DRG Study Section illustrates (Figure 1), the NIH has traditionally expected review groups to review whatever proposals are received by a given receipt date. In the past, allowances have not always been made for possible limits imposed by other responsibilities of review group members, or the effects of fatigue. The Study Team recommends that the importance of these factors be recognized explicitly, and that appropriate ceilings on application loads be set based on NIH review staff experience, in a centrally managed periodic review. Once established, NIH policy should require that these ceilings be honored.

The Study Team has drawn upon the recommendations of the NIH Executive Secretaries Review Activities Committee (ESRAC) and the suggestions of ESRAC workshop participants in formulating this proposal(1). Because each type of application requires a different level of effort, and because applications in different disciplines may be more or less time-consuming and difficult to review, ceilings can only be determined individually for each group.

5. THAT THE NIH CONSIDER WHETHER THERE ARE PEER REVIEW SERVICES TO NIH STAFF, ADVISORY COUNCILS, OR APPLICANTS WHICH MIGHT BE CURTAILED IF ALL OTHER WORKLOAD REDUCTION ALTERNATIVES SHOULD FAIL.

The NIH has continued to expand the services derived from or related to the peer review process for grant applications, in recent years, in the face of diminished resources and increasing review workloads. These services include providing the detailed technical information of review group critiques to investigators; reviewing, at the next scheduled review session, all applications received by or soon after a given receipt date regardless of the workload imposed; accepting an unlimited number of reapplications for a given research proposal if the original or subsequent applications did not receive an award; and assisting applicants to assure that review groups receive the most complete application for review.

Until about 1968, when research grant funds each year were adequate to make awards for most of the applications recommended for approval by the IRGs and Advisory Councils, very few grant applicants requested detailed information about the peer review critique of their applications. Those whose applications had been disapproved had other avenues of support or alternative careers readily available to them. Since that time, as NIH funds have ceased to grow rapidly (Figure 2) and the job market for researchers and academicians has become highly competitive, applicants have a much higher stake in the peer review assessment and funding of their research proposals. Alternative sources of support are scarce. Detailed information from the review critique can possibly enable the applicant to restructure the project and compete successfully. There are more reapplications, and most unfunded principal investigators demand detailed information about the review. Reviewers and executive secretaries must work harder to provide this detailed information. While the Study Team agrees that curtailing such services or providing less useful service is undesirable, this recommendation is included to emphasize that the preservation of the quality and reliability of reviews and recommendations *per se* must take precedence.

IV. POSSIBLE ALTERNATIVES FOR REDUCING WORKLOAD

Recommendation 5 urges that the NIH consider seriously some alternatives for controlling the review load of individual review groups and reviewers, but *only* under circumstances where needed resources cannot be provided to reduce workload. The Study Team has reviewed possible alternatives for reducing peer review workload which are other than trivial. These are:

A. The establishment of additional review groups in order to "split" or share the workload of groups which continuously receive more applications each round than they can review effectively. This is the substance of Recommendation 3, and we urge that it be done at once, where there are sufficient numbers of qualified experts to constitute the needed new groups.

B. Provision for junior executive secretaries, preferably new health scientist administrators, to work under the direction of the executive secretary for the group. This arrangement can be a useful training device for new executive secretaries and may serve, in some cases, to reduce the duration of review sessions and each reviewer's review load. The Study Team recommends that this approach be explored.

C. Provision for editorial services in DRG, or within each BID which reviews grant applications in order to assist the executive secretaries. Such editorial support would help to assure the quality of summary statements and conformity to standard formats and other NIH requirements. *The Study Team strongly recommends that such a system be considered.*

D. Short-term use of available IRGs within or across BID boundaries when such review groups, established for specialized programs, could easily absorb additional workload.

E. Creation of Special Initial Review Groups (SIRGs). This is discussed in another section of this report.

F. Contractor-conducted peer review. There are precedents for contracting out peer review. The Office of Life Sciences, NASA, for example, has contracted in the past with the American Institute of Biological Sciences for scientific and technical merit review of research proposals. Properly staffed and oriented contractor organizations can provide peer review of high quality. Contractor staff are substituted for agency staff, and do not count against the agency position ceiling. The dollar cost of contracting out more than a small part of the NIH grant peer review operation would be significant, however, and close NIH staff overview and coordination would be required; sparing of staff positions would be partial at best.

The NIH has every reason to preserve and upgrade the present system rather than to replace it. From the Wooldridge Committee to the Rand Corporation, most groups which have studied peer review have reaffirmed the value and quality of the system. It has served as a model for other agencies and programs to emulate. The Study Team recommends that the contracting alternative be reserved for special uses with appropriate NIH overview to assure conformity to NIH peer review policies and procedures.

G. Modification of peer review procedures to permit review groups and staff to absorb greater workloads. One such change has recently been implemented; with the change to the new fiscal year, the review cycle has been lengthened from six months on the average to eight months on the average. This action, while predictably not popular with applicants, does give executive secretaries more time in which to schedule site visits and prepare summary statements. Other possible modifications are:

1. To decrease the time and effort required to review each application by staff screening and rejection of incomplete or incoherent applications; to curtail discussion of applications judged by the primary reviewers to be either excellent or without merit; to curtail or eliminate site visits; etc. Casual observers of review meetings often make such suggestions, saying, for example, that too much time is spent by the primary reviewers in reading or paraphrasing the proposed project which all have in front of them.

   The responses from executive secretaries, and from grant peer review group members (see Table 16, Page 61 of Survey Report, Supplement A), suggest that full review of each application is generally held to be more important than a rapid rate of review which might be at the expense of the quality of the review. Provided a review group is working effectively, the opportunities to spare peer review resources through such review short-cuts are very limited, and best not pursued. Further staff initiatives to cull inadequate applications might be explored.

2. To curtail staff screening of applications by executive secretaries to assure that applications are complete and clear before they go to the review groups. This service to the applicant researcher is a useful function of the executive secretary, intended to assure that the applications are fairly reviewed rather than to save time or effort. It is a valuable service which NIH review staff renders to grant application principal investigators, as workloads permit. We do not recommend, except in case or dire necessity, that this staff assistance to the applicant be curtailed or abandoned.

3. To queue applications. This practice has been proposed by DRG in the past. All applications beyond the determined review capacity per round of the assigned review group would be held to the next review cycle. The NIH has rejected this proposal in the past, adhering to a long tradition of providing prompt review for all applicants whose applications reach the NIH on or before a given receipt date. Nevertheless, it is felt that queueing is preferable to acceptance of poor quality of review since it is essential that only meritorious projects are supported. From that perspective, the NIH can afford to reject queuing or any other such heroic, distasteful measures only if alternative ways of reducing or adjusting review workloads can be invoked.

4. To reduce the number of reapplications, after an application is reviewed but not awarded. The review workload can be contained somewhat if the number of such reapplications which will be accepted by the NIH in behalf of a given individual can be limited, perhaps to one per project. Since all investigators should have every opportunity to reapply in order to obtain support and a revised application is, in essence, a new application this alternative is not acceptable to the Study Team.

   A more acceptable alternative, where an unchanged application is resubmitted with the hope that it will compete successfully for funds the second or third time around, is to forward the application for BID consideration on the basis of the application's prior merit review. Applicants usually do benefit from the evaluation of the first application, however, and second or subsequent submissions are most often revised to take advantage of information from the earlier review.

V. OTHER WORKLOAD-RELATED FACTORS

There are factors which are crucial to the quality and consistency of NIH grant application peer review to which the NIH has not addressed adequate effort in the past. To improve the efforts in these areas would require assurance that the review workload per se of executive secretaries is not so large as to preclude the involvement of these key individuals in quality control activities. These activites include:

- Formal orientation and continuing education programs, and an adequate apprenticeship period for executive secretaries before assuming full responsibility for a review group;
- An effort to assure that the peer review process is reasonably consistent across the NIH, for such matters as the interpretation and use of the priority rating procedure, the concepts of "approval" and "disapproval," and the full participation of review group members in discussion.

  The Division of Research Grants has attempted to compensate for variability in the rating behavior of Study Sections, over time, by computing "normalized" scores. This represents an effective but indirect approach to the problem which has certian limitations. One direct approach would be to provide opportunities for executive secretaries to work together, to observe and learn from the review process of groups other than their own, to obtain appropriate training, and to have available information, for their own use and for their reviewers, which would provide basic definitions, review objectives, procedures, and criteria, and which would be reviewed with each new reviewing or staff member.
- Common standards and procedures across the NIH for project site visits, criteria for determining when a site visit is needed, and the content and format of site visit reports.

Many of these suggestions are discussed elsewhere in this report.

VI. THE "SUNSHINE LAWS"

The requirement for more detailed documentation of review recommendations and more careful management and retention of review information, generated by the Freedom of Information Act and Privacy Act, has increased the complexity of the peer review workload and has added somewhat to the effort required to process each application. These laws are discussed in another section of this report.

IMPACT OF REVIEW WORKLOAD ON QUALITY OF INITIAL MERIT REVIEW

References

1. Kaufman, A. and Schiaffino, S. Memorandum: ESRAC Recommendations from Extramural-Collaborative Retreat and ESRAC Summary Statement Workshop. (1/7/76).

2. National Cancer Act of 1971 [Public Law 92-218].

Fig. 1

TOTAL NUMBER OF APPLICATIONS (R-01 AND RCDA) REVIEWED BY ALL DRG STUDY SECTIONS (MARCH 1969 - OCTOBER 1976)

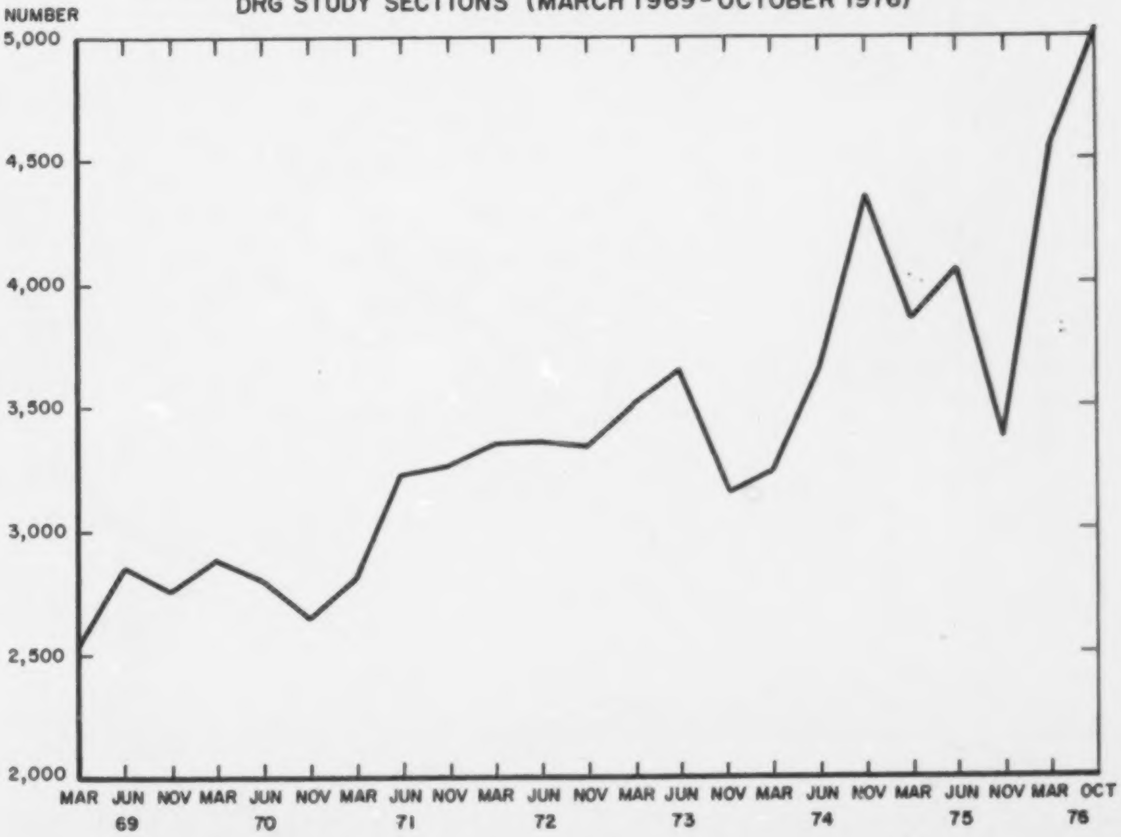


Fig. 2

AVERAGE NUMBER OF APPLICATION (R-01 AND RCDA) REVIEWED PER MEETING OF DRG STUDY SECTION (MARCH 1969-OCTOBER 1976)

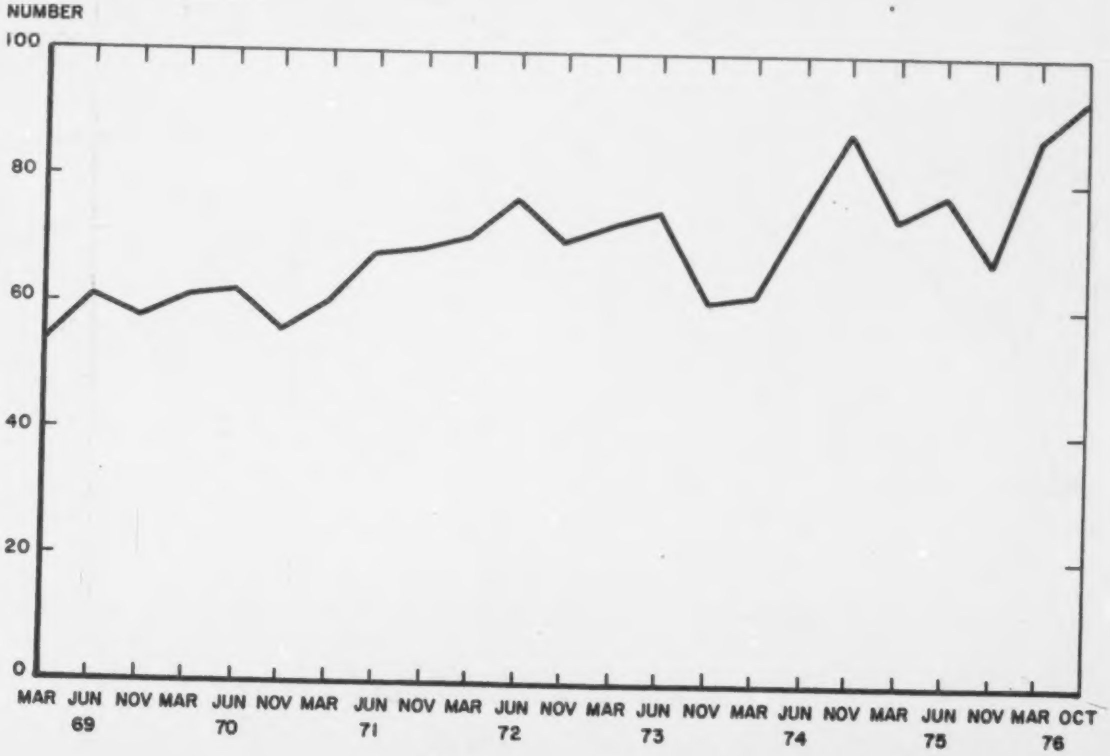
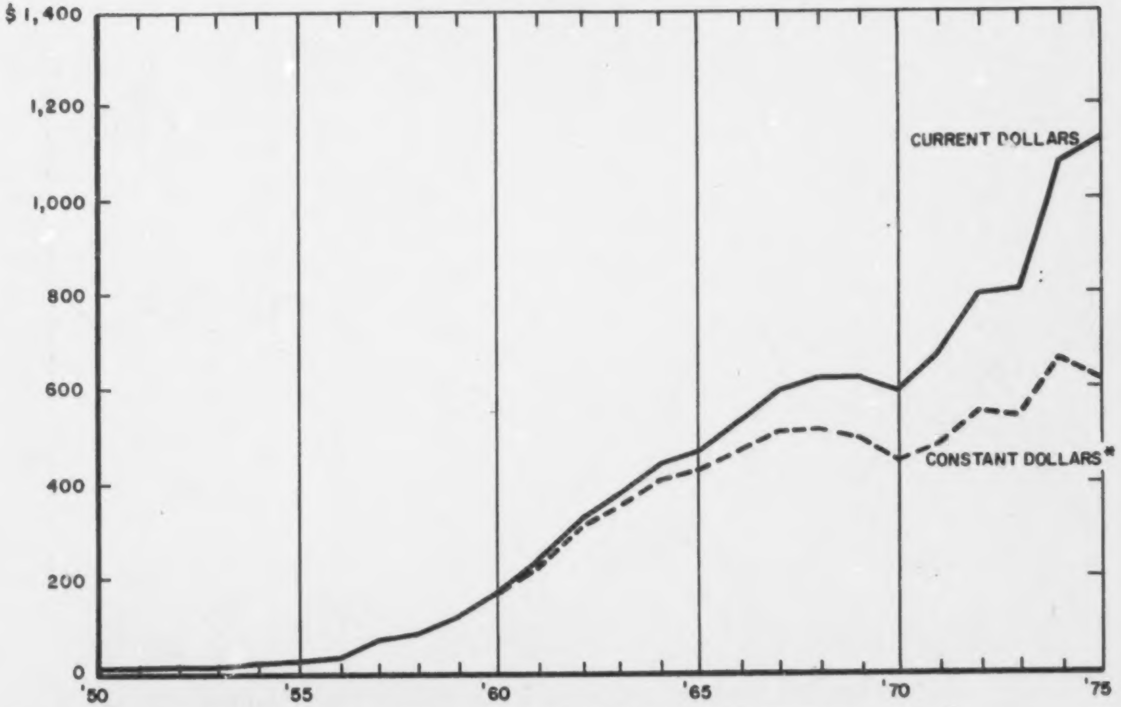


Fig. 3

NIH RESEARCH GRANT AWARDS
FISCAL YEARS 1950-1975



NOTE: NIH IS DEFINED THROUGOUT AS PRESENTLY CONSTITUTED. EXCLUDES RCP PROGRAMS.
*BASED ON THE GNP IMPLICIT PRICE DEFLATOR, FY 1958=100.
SOURCE: NIH, DRG, STATISTICS AND ANALYSIS BRANCH.

NIH-WIDE STANDARDS AND GUIDELINES FOR PEER REVIEW PROCEDURES

NIH-WIDE STANDARDS AND GUIDELINES FOR PEER REVIEW PROCEDURES

In order to provide a legal mandate for the NIH system of scientific peer review of grant applications, Congress amended the Public Health Service Act in 1974 by adding a new Section 475. This section directs the Secretary of DHEW to require, by regulation, appropriate scientific peer review of applications for biomedical and behavioral research grants under the said Act. Section 475 states that the regulations shall, to the extent practicable, require peer review to be conducted in a manner consistent with the NIH system in use at the time Section 475 was enacted, and by peer review groups performing such review on or before that date.

In accordance with the aforegoing statutory mandate, DHEW has published a notice of proposed rulemaking in the Federal Register (Appendix F-2) which sets forth proposed regulations regarding the use of and criteria for NIH grant application peer review.

Although several of the BIDs have developed guidelines for their own use, NIH-wide guidelines and standards governing specific operating procedures in peer review are incomplete. It seemed appropriate to determine whether general guidelines, applicable NIH-wide, are needed, and if so, to develop the appropriate documents. Accordingly, the Associate Director for Extramural Research and Training and the Grants Peer Review Study Team asked the Executive Secretatires Review Activities Committee (ESRAC) to conduct such a study by collecting and identifying all current written policies and procedures developed by NIH and the various BIDs and to present recommendations regarding their applicability for general use by all BIDs. The ESRAC report is due early in 1977. It is expected that it will provide NIH with the documentation necessary to determine the need for additional NIH-wide guidelines or standards beyond those already contained in the proposed regulations.

The Study Team believes that an important outcome of the ESRAC study will be the identification, for all BIDs, of the standards and guidelines utilized by particular individual BIDs, some of which may be appropriate for utilization NIH-wide. It is recognized that there may be instances which require a particular BID to have certain policies based on its own particular needs. Nonetheless, it is felt that by providing BIDs with information as to the manner in which each of them functions, all will benefit from the knowledge gained by their collective experience.

Furthermore, it is a sine qua non that the quality of peer review and of administrative practices followed by BIDs in regard to research grants and other award instruments is directly related to the staff which has the various responsibilities. In order to provide and maintain high quality staff activities and to assure its continued functioning at the highest level, an on-going quality control program is essential. Central to this is the need for formal training both for new staff and for those whose duties may become routine. In addition, frequent discussion groups for staff, during which open and free communication can occur are also important training experiences.

The quality of peer review is dependent not only on the professional NIH staff, but more importantly, on the advisory groups without whom the system could not function. It should be recognized that education of members of Initial Review Groups (IRGs) and Councils is essential and is a continuous process. Although guidelines are available in the form of a handbook for advisory groups, orientation sessions for both new and old members of IRGs and Councils are important in order to emphasize and reemphasize the responsibilities of each individual in regard to review and the need for group interaction.

RECOMMENDATIONS

1. THAT THE PROPOSED PEER REVIEW REGULATIONS BE FINALIZED AS SOON AS POSSIBLE.

2. THAT NIH STANDARDS AND GUIDELINES BE PREPARED OR REVISED AS SOON AS POSSIBLE AFTER CONSIDERATION AND EVALUATION OF RECOMMENDATIONS MADE IN THE STUDY BY ESRAC.

3. THAT, IN ORDER TO MAINTAIN AND IMPROVE THE LEVEL OF EXCELLENCE OF THE GRANTS PEER REVIEW SYSTEM AT NIH, A PERSONNEL POSITION SHOULD BE ESTABLISHED WITHIN THE OFFICE OF EXTRAMURAL RESEARCH AND TRAINING TO PROVIDE FOR QUALITY ASSURANCE OF THE SYSTEM. SUCH A PERSON SHOULD HAVE STAFF RESPONSIBILITY FOR MATTERS PERTAINING TO THE FORMULATION, DEVELOPMENT, INTERPRETATION, AND APPLICATION OF POLICIES RELATING TO THE ASSIGNMENT AND OVERALL SCIENTIFIC AND TECHNICAL REVIEW OF RESEARCH GRANTS AND MANPOWER DEVELOPMENT PROGRAMS.

4. THAT TRAINING CURRICULA BE DEVELOPED BY NIH FOR EXTRAMURAL PROGRAM AND REVIEW STAFF IN ORDER TO PROVIDE ORIENTATION AND TO REFRESH AND REITERATE PRINCIPLES CONCERNING THE PHILOSOPHY, OBJECTIVES, AND PROCEDURES FOR PEER REVIEW.

5. THAT OPEN FORUMS AND WORKSHOPS FOR PROGRAM AND REVIEW STAFF BE ESTABLISHED ON A CONTINUING BASIS SO AS TO ENCOURAGE AND IMPROVE EXCHANGE AND COMMUNICATION OF IDEAS CONCERNING ISSUES RELEVANT TO THE PEER REVIEW SYSTEM.

6. THAT THE DIRECTOR, NIH, THE BID DIRECTORS, AND THEIR STAFFS HOLD ORIENTATION SESSIONS ANNUALLY FOR ALL IRG AND COUNCIL MEMBERS, PARTICULARLY NEW MEMBERS, IN ORDER TO PLACE PEER REVIEW IN PERSPECTIVE AND TO INFORM REVIEWERS OF THEIR FUNCTIONS, DUTIES, AND RESPONSIBILITIES.

NIH-WIDE STANDARDS AND GUIDELINES FOR PEER REVIEW PROCEDURES

References

1. Orientation Handbook for New Members of Study Sections. Division of Research Grants, National Institutes of Health (1976).

PEER REVIEW OF PROGRAM PROJECT AND CENTER GRANTS

PEER REVIEW OF PROGRAM PROJECT AND CENTER GRANTS

Peer review procedures for program project and center grant applications have received and continue to receive intensive examination by NIH staff to assure that the same general principles of review are followed for the complex large grant applications as for the traditional investigator-initiated research grant application.

The perceptions of the biomedical community regarding program project and center grants focus on a number of issues, some of which relate to peer review of such grant applications and others which are more peripherally, if at all, related to peer review. Of those who addressed the issue of program projects and center grants in letter responses to the Study Team or in testimony at the public hearings, the following concerns were most frequently mentioned:

(a) the poorer quality of peer review of program project and center grant applications as compared to that of the traditional scientific research grant applications;

(b) inadequate separation of review and program functions among BID staff which leads to bias in the review as a result of pressure in reviewer selection;

(c) the appearance of conflicts of interest for or against a program project or center grant among site visitors reviewing such applications;

(d) adequate balance of funds between traditional and program project and center grants and the increasing allocation of funds for the latter;

(e) the lengthy duration of committed support for program project and center grants, thus closing off funds available for initiation of new regular grants.

NIH review group members also were concerned about the adequacy of the review of program project and center grant applications. The survey questionnaire revealed that, whereas 97% of the respondents thought that the review of traditional research grants was excellent or good, a significantly lower percentage (78%) of the respondents thought that the review of program project and center grant applications was excellent or good (Supplement 1).

The proportion of research grant funds awarded for program projects and centers varies considerably among the BIDs. Nevertheless, it should be pointed out that, in FY 1976, all BIDs except the Division of Research Resources (DRR) and the National Institute of Environmental Health Sciences (NIEHS) awarded more than half of their research grant funds for individual research projects. The National Institute of Allergy and Infectious Diseases (NIAID) and the National Eye Institute (NEI) awarded 91% of their research grant funds for research projects, followed by the National Institute of Arthritis, Metabolism and Digestive Disorders (NIAMMD), 87%; National Institute of General Medical Sciences (NIGMS), 77%; National Institute of Child Health and Human Development (NICHD), 72%; National Institute of Neurological and Communicative Disorders and Stroke (NINCDS), 68%; and the National Institute of Dental Research (NIDR), 61%. The National Cancer Institute (NCI), 59%; National Heart, Lung, and Blood Institute (NHLBI) 54%; and the National Institute of Aging (NIA), 54%, awarded slightly more than one-half of their research grant funds for research projects. The NIEHS divided its awards almost evenly between research projects and program projects and centers. Almost all of DRR research grant funds (excluding General Research Support Grants) were awarded for center grants reflecting the particular mission of that organization.

Peer review procedures for program project and center grant applications have been the subject of a further recent study by an NIH staff group (Appendix G). Other studies are also in progress to examine a series of issues relevant to these complex grants. The concerns expressed by the biomedical community reinforce the Study Team's recommendation that the aforementioned studies be continued. In addition, it is strongly recommended that there be strict adherence to the principles of peer review in order to assure the highest quality of review for these complex grants.

PEER REVIEW OF PROGRAM PROJECT AND CENTER GRANTS

References

1. Subcommittee Report on Peer Review of Large Programs at the NIH. (Program Project and Center Grant Task Force) (1975).

2. Report of the Termination Subcommittee and Report of the Subcommittee on Utility and Effectiveness. (Program Project and Center Grant Task Force) (1975).

3. Report of Institutional Profiles Subcommittee. (Program Project and Center Grant Task Force) (1974)

4. Report of the Subcommittee on Management Practices. (Program Project and Center Grant Task Force) (1973).

REVIEW OF BUSINESS MANAGEMENT PRACTICES

REVIEW OF BUSINESS MANAGEMENT PRACTICES

In developing its assessment of the peer review process and preparing its report, the Grants Peer Review Study Team (GPRST) felt that a study of review procedures, particularly for large, complex, multi-faceted grants, in regard to the business management practices of the institutions applying for such grants was essential. Accordingly, a subcommittee on business management practices was established and performed an in-depth study which is endorsed by the GPRST and is incorporated in this section.

I. Background

The Subcommittee was charged with the responsibility of studying all aspects of the business management issues associated with the peer review process and making recommendations based on its findings.

In considering various approaches to meeting the assignment, the Subcommittee, in its first two meetings, made several basic determinations, such as:

A. Since the issue at hand concerned peer review and not staff review, the study would actually deal primarily with the NIH use of outside business management consultants at the level of the initial review group or project site visit team. As intended here, the term, business management consultant, relates to any individual who, because of background—training and experience—is qualified to provide expert evaluation and advice in assessing grantee institutional business management systems/areas such as fiscal administration, budget planning and execution, personnel, procurement, facilities management, property, etc.

B. The grant applications that do feature or would appear to require business management input as a part of the peer review process would generally be of the program project and center types or those involving consortium arrangements and, accordingly, the study would be essentially limited to these.

C. It was recognized that, in the initial review of the larger and more complex grants, there is often the need for input from other non-scientific reviewers whose backgrounds and expertise fall into the area of senior level program management, i.e., academic deans, etc. Further, even though there can occasionally be some overlap of program management and business management consultant review activity, depending upon individual qualifications and specific circumstances, no attempt would be made to include the role of the program management consultants in the study.

D. While substantial extramural operational background experience was present among the membership of the subcommittee, a great deal of informational input from outside would be required to develop a complete picture of the NIH involvement and/or commitment in this area.

II. Methodology

The major fact-finding activities of the group were pursued in a series of inter-related phases with the various inputs building upon one another. The primary efforts were as follows:

A. A preliminary search was made for NIH documents (Manual Chapters, etc.) which might have reference to the business management aspects of peer review and, from within the organizations represented by the membership, a collection was made of any pertinent BID peer review procedural directives or guidelines.

B. Since the above provided only an extremely limited indication of what was being done at NIH, it was determined that a standardized questionnaire should be developed for soliciting information from each of the BIDs. The questionnaire was designed to generate specific data on the extent of non-NIH business management consultant utilization (see Appendix H-1). Along with responses to eleven questions, the document requested submission of copies of any written guidelines routinely provided to business management consultants prior to participation in site visits and, also, samples of Project Site Visit (PSV) reports and/or Initial Review Group (IRG) Summary Statements reflecting specific contributions of the business consultants. The responses to the questionnaire were summarized and studied (see Appendix H-2).

C. While the data were being collected from the NIH BIDs, it was decided that an attempt should be made to obtain input on similar activities which might be involved in the review of grants by some other agencies with missions comparable to NIH. At a meeting arranged with staff of the National Science Foundation and the Alcohol, Drug Abuse, and Mental Health Administration, comments of the visitors were quite interesting but indicated that little or no business management expertise is built into the grant review processes of the two agencies (see Appendix H-3).

D. It was felt that there would be potential value in obtaining input from a representative number of individuals who have performed for NIH in a business management capacity within the grant peer review process. An all-day meeting was arranged with seven such consultants with NIH experience ranging from very extensive to comparatively limited (see Appendices H-4 and H-5).

The session was felt to be quite beneficial, providing ample opportunity to learn much of the way in which these people view, among other things: (1) the role they are called upon to perform; (2) the degree of receptivity to their efforts; (3) the type of report which should be written; and (4) the possibility for changes in approach which might make their participation more effective.

E. Having heard extensive commentary by this group of consultants, the Subcommittee determined that it would be of interest to review two or more examples of IRG Summary Statements and Project Site Visit (PSV) reports to which each of the individuals had contributed. With the help of at least six BIDs, sixteen such documents were collected, duplicated, and reviewed. In an attempt to display possible differences in approach, examples were selected where the individual consultants had worked for different BIDs or participated in reviews of different types of grant applications.

F. Because of the important role played by the NIH Executive Secretaries in the total grants review process, it was decided to obtain input from a representative number of such NIH staff on the same issues which had been discussed with the business management consultants (see Appendix H-6). To allow for freedom of expression, arrangements were made for four separate meetings with appropriate staff responsible for review from DRG, NICHD, NCI and NHLBI. With real differences of personal opinion being shared by the Executive Secretaries, these meetings were found to be very enlightening.

G. As a final fact-finding effort, the Subcommittee decided to approach another group of individuals believed to be uniquely qualified to react to questions on the role of the business management consultant in peer review. This would be a number of senior NIH scientific consultants who have either chaired permanent or ad hoc initial review committees or project site visit teams, or both. To contact such individuals with the least inconvenience to them, it was determined that a telephone survey would be made, utilizing conference calls so that two or three Subcommittee members could speak with each of the chairmen. A standardized list of questions was developed in advance to use as a guide in querying the eight chairmen with whom conference call arrangements were made. This approach worked quite well and much helpful information was obtained (see Appendix H-7).

III. RECOMMENDATIONS AND DISCUSSION

No listing of general findings has been presented previously because each of the recommendations that follow will have an accompanying statement of rationale. These statements will include significant evidence of what was found by the Subcommittee in its various phases of information-gathering. The recommendations have been developed because of the strong conviction that the actions proposed are critically needed to formalize an NIH position on an important aspect of the grants peer review process, which, in the past, has been left largely to whim or chance. The recommendations are:

A. THAT IT BE RECOGNIZED AS AN NIH POLICY POSITION THAT THE USE OF BUSINESS MANAGEMENT CONSULTANTS AS REVIEWERS IS A NECESSARY ADJUNCT TO THE SCIENTIFIC REVIEW OF THE LARGE, COMPLEX AND MULTIFACETED PROGRAM PROJECTS, CENTERS, OR THOSE GRANTS INVOLVING CONSORTIUM ARRANGEMENTS.

The evidence developed through the questionnaire indicates that the use of such consultants in the review of program project and center grant applications ranges across the multiple BIDs from "never" to "almost always." These are perhaps not equal extremes, but it is concluded that NIH should present more of a reasonable "middle ground" picture on this issue with, at least, no BID being permitted to determine on its own "never" to avail itself of the benefit of such management expertise. The discussions with the business management consultants, the Executive Secretaries, and the IRG or PSV chairmen provided ample support for the basic value of augmenting the highly specialized scientific review with appropriate evaluation of business management practices.

B. THAT THE PRINCIPAL CRITERIA USED TO DETERMINE THE NEED FOR BUSINESS MANAGEMENT CONSULTANT SUPPORT BE RELATED TO THE ORGANIZATIONAL OR ADMINISTRATIVE COMPLEXITY EXISTING IN ANY PARTICULAR GRANT APPLICATION.

This recommendation is based on the understanding that there is no simple way of determining when it would be appropriate to utilize business management expertise in reviewing an application. Certainly, sheer size of the program or proposed dollar level cannot be used as hard and fast criteria. It is in keeping with the many inputs received to say that each application must be reviewed on its own with a judgmental determination being made as to complexity.

C. THAT, CONSIDERING THE NATURE OF THE TRADITIONAL INVESTIGATOR-INITIATED RESEARCH PROJECT APPLICATION AND ITS REVIEW, IT BE RECOGNIZED THAT THE USE OF BUSINESS MANAGEMENT CONSULTANTS WOULD NOT USUALLY BE CONSIDERED NECESSARY OR APPROPRIATE.

Such a position was uniformly held by all individuals consulted. However, it was expressed with the understanding that the individual research grant application could represent a very large and complex project, particularly if it involved a consortium arrangement, and might be equally in need of expert business management review.

D. THAT NIH DEVELOP A POLICY ISSUANCE CONCERNING THE ROLE OF AND NEED FOR ASSESSMENT OF BUSINESS MANAGEMENT IN THE REVIEW OF THE LARGE PROGRAM PROJECT AND CENTER GRANT APPLICATIONS.

Along with the three previous recommendations, this is a natural follow-up since there is currently no NIH policy which speaks to the issue of business management review. It is felt that most of those consulted endorse such a proposal as long as there is no attempt to narrowly define the circumstances under which management consultants are to be used in the peer review process.

E. THAT NIH DEVELOP UNIFORM GUIDELINES TO BE FOLLOWED BY THE BUSINESS MANAGEMENT CONSULTANTS IN THEIR ROLES AS MEMBERS OF PROJECT SITE VISIT TEAMS AND ADVISORY GROUPS.

In spite of the extensive use of business management consultants by certain components of NIH, there have never been any NIH-wide guidelines developed to indicate what is expected of them. Those with whom the Subcommittee met indicated that they had developed their own approaches to doing the job and were sometimes left wondering whether they were turning in a satisfactory piece of work. The Executive Secretaries and PSV chairmen confirmed that they provided little verbal guidance, if any, to the business management consultants. As might be expected, the review of multiple summary statements and PSV reports indicated vast differences in approach. There was a uniformly positive response from all quarters to the idea of NIH guidelines being developed.

1. That the consultant's function should be to review and recommend primarily on the adequacy of business management practices and arrangements (including soundness of budgetary planning) for the program project or the center itself.

2. That the business management consultant normally should not be concerned with exploring the history, current financial status, or administrative adequacy of the institution as a whole unless special situations call for it.

3. That, in following the standard site visit approach of separating away from the scientific review, the business management consultant should concentrate on dealing with institutional/administrative staff members who will be directly involved with the project.

4. That flexibility should be permitted so that, depending upon the individual and the specific circumstance of his/her participation, the business management consultant could be asked to contribute to other administratively oriented aspects of grant application review, such as proposed

procedures for decision-making and resource allocation within a program project or plans for controlling access to and use of shared facilities and services in a core center or resource grant.

E.1, 2, 3, and 4 have been tied together because of similarity and relationship to the basic recommendation E concerning development of performance guidelines for the business management consultant. These four recommendations present new concepts which are based on general concurrence from the group of administrative consultants. Regardless of the historical background for it, some of the BIDs have been responsible for fostering an institutional review approach on the part of the management consultants which frequently expends much of their effort in areas of little profit. It is apparent that the great bulk of the large grants are awarded to the larger and well established institutions. Further, in looking at the "large grants" picture, it is understandable that many of these institutions might have as many as 10, 20, or more competitive reviews on the large grant applications each year. Therefore, as a general rule, it may be safe to say that there is little real value in having the consultants devote significant time to interviewing top institutional officials, i.e., presidents, vice-presidents for administration or research, comptrollers, etc., and writing much into their reports on institutional history, financial status, and administrative adequacy. It is felt that there is a very important role for the consultant in evaluating thoroughly various business management aspects of the level of the program project and/or center itself. This may mean dealing extensively with a number of mid-level management officials, but this is where the real knowledge on the extent of planning and proposed operational approaches can be obtained. It is also the level where the consultant may be able to provide some real assistance to the grantee staff in making suggestions on how to overcome identified inadequacies or proposing alternative approaches in areas where he might feel it appropriate. It is clear that there is a need for maximum benefit to be obtained from the participation of qualified management personnel. However, another misuse practiced by some of the BIDs should be mentioned. From a review of summary sheets/PSV reports and from the comments of the business management consultants themselves, it is apparent their maximum efforts are, at times, directed toward tracking and recording the multiple budgetary adjustments made in the course of a PSV on the large grants. Such activity, when combined with the more responsible role, is acceptable; but, if it is considered as the primary reason for having such individuals participate in the review, then this represents a serious misuse of such expertise.

F. THAT BUSINESS MANAGEMENT CONSULTANTS SHOULD CONTRIBUTE TO OVER-ALL RECOMMENDATIONS ON PROJECT SITE VISITS AND AT INITIAL REVIEW GROUP MEETINGS, BUT THEY SHOULD NOT VOTE ON "APPROVAL VS DISAPPROVAL" OR GIVE A PRIORITY RATING UNLESS, IN THE JUDGMENT OF THE EXECUTIVE SECRETARY, THE TYPE OF PROJECT INCLUDES SIGNIFICANT NON-SCIENTIFIC ASPECTS CLEARLY WITHIN THEIR PROFESSIONAL EXPERTISE.

The question of voting was a very intriguing one. The business management consultants themselves were somewhat ambivalent on the question of whether they should vote. There was an indication that some abstain from voting even though they are almost routinely asked to do so. Recognizing that the peer review is clearly an overall evaluation of scientific merit, an attempt was made to determine the basis on which the management consultants would vote. It was felt that, if the vote was based primarily on their administrative evaluation, this would be questionable; if they were voting on what they thought was a consensus of the scientific reviewers' opinions, this would also be questionable, but for vastly different reasons. It was difficult to get any kind of uniform response from the management consultants, so the question was also raised with the Executive Secretaries and the committee chairmen. Here, the question was more difficult because they were being asked to speculate on the basis of the management consultant's vote. The responses were even more variable, but generally led to a discussion of the fact that it really did not matter because the management consultants represented such a limited minority in the typical voting situation that their individual vote would have no impact anyway. Many of the chairmen reversed their previously stated position that the management consultants should have the vote when asked how they would feel if the total participation in the review activity involved only six or seven individuals, rather than 16 to 22. The strong feeling developed that, while the management consultants could provide significant input or findings worthy of serious consideration by the whole group of scientific reviewers in looking at the total application package, the business consultants should not have the option to vote. An exception to this rule would be made if the Executive Secretary of the IRG or PSV team makes a determination that the nature of the project includes significant non-scientific aspects clearly within the professional expertise of the business management consultant. In this case, the consultant could exercise a full vote on approval vs disapproval and on the priority score.

G. THAT BUSINESS MANAGEMENT CONSULTANTS SHOULD PREPARE A SPECIFIC PORTION OF PROJECT SITE VISIT REPORTS AND/OR IRG SUMMARY STATEMENTS WHICH SHOULD BE RECOGNIZED AS SEPARATE FROM SCIENTIFIC REVIEW AND EVALUATION, BUT SHOULD BE CAREFULLY CONSIDERED IN REACHING RECOMMENDATIONS FOR EACH PROJECT

This recommendation is closely related in some ways to the previous one on the "vote." Again, recognizing the basic responsibility for scientific merit assessment by the initial reviewers, it was felt that the business management evaluation should be kept distinct from the scientific evaluation. The business management consultant's portion of the report could be critical to the overall judgment exercised by the scientific reviewers of the total program, but it should be clearly separate from the scientific critiques of program segments. Most of those individuals questioned agreed completely with this position.

H. THAT PROCEDURES BE DEVELOPED WHICH WOULD ALLOW THE REPORTS OF THE BUSINESS MANAGEMENT CONSULTANTS, INCLUDING SPECIFIC RECOMMENDATIONS FOR MANAGEMENT IMPROVEMENT, TO BE MADE AVAILABLE TO THE APPLICANT INSTITUTION BY THE BID AT AN APPROPRIATE TIME AFTER COMPLETION OF THE REVIEW.

This recommendation relates to the rationale for E.1, 2, 3, and 4 (content) and G (format) above. With appropriate guidelines for the management consultants, it is felt that many more reports (than are currently seen) could provide significantly helpful advice for the management staff of the grantee institutions. If this is the case, then that information should be readily transferable by staff of the awarding component to appropriate personnel at the institutions where it could be utilized. Implementation of this recommendation is not now proposed, but rather it is suggested that a standardized report formating or other ideas should be considered to make this recommendation feasible. The grants management officer should be responsible for coordinating this activity within the BID.

I. THAT GRANTS MANAGEMENT OFFICERS OF THE BIDs PARTICIPATE WITH THE IRG EXECUTIVE SECRETARIES IN THE DETERMINATION OF A NEED FOR, AND IN SELECTION OF, BUSINESS MANAGEMENT CONSULTANTS FOR THE PROJECT SITE VISIT TEAM INVOLVEMENT.

Under recommendation B, reference was made to the fact that the need for business management consultant support should be related to the organizational or administrative complexity of the individual grant application. With the grants management officers' background experience and concerns for smooth pre- and post-award administration, and the fact that they sometimes have different ways of looking at issues (from program management staff), it seems reasonable and logical to propose their participation with the Executive Secretaries in making the determination of need. Likewise, because of their continuing day-to-day involvement with business management staffs of grantee institutions, it was felt that they could be particularly helpful in selection of the business management consultants.

J. THAT, WHENEVER POSSIBLE, BID GRANTS MANAGEMENT STAFF MEMBERS SHOULD ACCOMPANY THE SITE VISIT TEAMS ON THE REVIEWS OF THE COMPLEX PROJECTS, SERVING AS STAFF RESOURCES ONLY.

This recommendation came primarily from discussions with the Executive Secretaries and the IRG/PSV chairmen. Some feeling was expressed that grants management staff could serve in lieu of business management consultants, performing the same role, except that they would not be considered as actual members of the PSV team and, of course, would not vote. However, the majority did not favor this approach for a variety of reasons, not the least of which was related to a possible conflict of interest. Here, an analogy was drawn to the impropriety of utilizing an extramural BID program manager as a scientific reviewer. Perhaps a more practical reason for negativity was based on the belief that the grants management staff were, in general, not experienced in grantee institution management and could not really do the same type of job as the business management consultant who "knew the territory" and would be able to "open necessary doors." While agreeing, it was felt that the basis for the above recommendation is: (1) that the grants management staff can and do serve as very valuable staff resources because of their intimate knowledge of grants policies and procedures; and (2) that the firsthand exposure to the institutional staff, facilities, and environment at the time of review would provide the grants management staff with many insights which could be of great value in carrying out later administrative responsibilities in the event a grant is awarded.

IV. Conclusions

In FY 1975, NIH obligated approximately $416 million for large program project and center grants. It is clear that NIH-wide policies and procedures should be developed specifying business management techniques to be employed in the grant peer review process. At present, no such directives exist and BID Executive Secretaries, business management consultants, and IRG or PSV chairmen have no uniform guidance in this important area.

THE ROLE OF PEER REVIEW IN SUPPORT OF UNORTHODOX, INNOVATIVE RESEARCH

THE ROLE OF PEER REVIEW IN SUPPORT OF UNORTHODOX, INNOVATIVE RESEARCH

In any process dealing with the support of research and development there must be provision for the identification of unusual and innovative approaches. Creative and unorthodox avenues of investigation may herald great leaps forward in concept; they may represent a turning point in research directions; or they may, all too often, be fruitless attempts to overcome obstacles to further progress.

I. DISCUSSION

Within NIH, as in the medical and scientific community at large, there is concern that the peer review system favors the support of traditional, conservative, orthodox research and that this is at the expense of innovative, unorthodox research. This perception is further complicated by the view held by many that innovative, especially creative research, is usually conceived by young investigators whose research ideas are usually untried, may not be well received and the significance of which not even be recognized. Consequently, such research applications are frequently disapproved or, if approved, at a priority which is not fundable. We are certain that this course of events does occur but we do not know how often and in what circumstances it is most likely to occur. Moreover, the Study Team does not believe there is any ineluctable relationship between youth and creativity.

One of the criteria used by IRGs in their scientific and technical merit review involves an assessment of the probability of success of the proposed project. If the concept is truly innovative and if the approach to the problem is unorthodox, then the reviewers are confronted with a dilemma. A proposal that involves an idea that is innovative or unorthodox requires an assessment of probable success and this may assume even greater weight and significance. Often, the evidence indicating probable success is limited and, therefore, many such applications are not recommended with enthusiasm. And yet, most reviewers are truly eager to find, identify, and recommend support for fresh new ideas. If an application contains a new or even unconventional approach to a problem and if it is well conceived in its component elements, IRGs will generally recommend support even though they believe that the likelihood of probable success may be marginal. Indeed, this eagerness may lead to a generous assessment of probable success in order to assure a favorable recommendation for support. If, however, the proposal is poorly conceived and/or inadequately prepared, IRGs are less likely to make a generous or favorable assessment of probable success of the project, and the ultimate recommendation may well be disapproval. Thus, in these circumstances, the applicant would seem to have a special responsibility to write clearly, explain fully, and, if possible, report the results or trends of pilot studies. In short, to compete successfully the innovative and unorthodox proposal should be buttressed by an especially well prepared application.

It should be noted that the NIH mechanisms of research support represent investments designed to attain a better understanding of disease processes that may lead ultimately to improvements in health. They do not represent investments in individuals only, in people per se—even those who are expecially creative and innovative. The investment is in people only to the extent that they conceive research ideas that may lead to the aforementioned results.

The membership of NIH peer review groups is seen by some as a force tending to preserve orthodoxy. The members are selected because of their competence in research, their contributions to biomedical knowledge, their stature, and their willingness to devote long hours to these arduous review tasks. Some believe that, by definition, they are "the establishment." They represent, in the aggregate, the current state of the art, the current vanguard of research—but not tomorrow's. These tendencies toward orthodoxy do indeed exist but they are offset by several factors: the efforts to have IRGs reflect a broad range of research viewpoints; the appointment of constructive, critical, enthusiastic investigators to review groups; the deliberate attempts to have outstanding young investigators on initial review groups; and the eager competition within review groups to detect and recognize the innovative, unorthodox proposal that may open new and promising avenues of investigation.

Certainly, the most subtle and most pervasive influence that fosters the support of safe, orthodox research, as opposed to the creative and innovative, is the limited availability of funds. As the dollars available for research support become more and more limited, there is a tendency (almost inevitable and often uncontrollable) to invest in the "safe bet." In such circumstances, advisors, consultants, staff members, and program managers avoid the high risk, the nonconformist, innovative proposal. Indeed, chronic or recurrent shortages of funds may indicate, and may cause, the demise of a truly vital research program.

Because so many individuals associate especially creative, innovative research with new and/or young investigators, the question of peer review as it relates to the support of new investigators is relevant here. Data derived from the study conducted by Douglass and James(1) indicate that, for the period from 1966 to 1972, approximately ten percent of all principal investigators on research project grants were being supported by NIH for the first time. Unpublished data for the period, 1972 to 1975, indicate that the percentage has remained about the same: about 53 percent of all new research project awards were received from investigators entering the NIH competitive system for the first time. Furthermore, in most Institutes the success (i.e., funding) rate for the younger investigators is better than that experienced by the older applicants. It should be noted, however, that the most critical and useful index for the success of the peer review system is the quality of research that has been supported over the years, and the advances in our knowledge which have come about from such support.

How can the especially innovative research proposal be identified? Those who submit what they consider to be creative, unorthodox research applications should be encouraged to identify their research as innovative; they should be required to state clearly and to document fully the innovative aspects of their proposal and the significant consequences that may emerge if support is provided.

The Study Team is of the opinion that the initial review groups have a particular responsibility in the identification of this type of innovative research. This requires perception, reflection, and the provision of adequate time for discussion. It is not easy to accomplish in circumstances where a review group is overburdened and tired. It is convinced that the IRG members are truly dedicated to such efforts. In fact, they seem to share the joy of discovery when they can identify and describe the creative insight, the new approach, or the truly innovative proposal. They are men and women of broad vision who are anxious to ensure that the present system provides for the adequate recognition and support of innovative research.

II. RECOMMENDATIONS

1. THAT NIH REQUIRE THE APPLICANT TO IDENTIFY AND SUPPORT IN DETAIL THE CONTENTION THAT THE RESEARCH PROJECT BEING PROPOSED IS ESPECIALLY INNOVATIVE;

2. THAT INITIAL REVIEW GROUPS BE REQUESTED TO IDENTIFY APPLICATIONS THEY CONSIDER TO BE ESPECIALLY CREATIVE OR INNOVATIVE, WHETHER OR NOT THE APPLICATIONS WERE SO IDENTIFIED BY THE APPLICANT;

3. THAT IRG MEMBERS (AS A GROUP OR INDIVIDUALLY) BE ENCOURAGED TO PREPARE A STATEMENT IN ADDITION TO THE REGULAR SUMMARY STATEMENT POINTING OUT THE INNOVATIVE ASPECTS OF THE APPLICATION AND ITS SIGNIFICANCE. THIS STATEMENT IS TO BE PREPARED REGARDLESS OF WHETHER THE APPLICATION WAS RECOMMENDED FOR APPROVAL OR DISAPPROVAL; THESE APPLICATIONS WILL BE FLAGGED FOR SPECIAL CONSIDERATION BY BID STAFF AND/OR ADVISORY COUNCIL;

4. THAT NIH CONSIDER THE FEASIBILITY OF DEVELOPING AN EXPERIMENT INVOLVING LIMITED SUPPORT FOR CERTAIN SPECULATIVE, HIGH-RISK, INNOVATIVE RESEARCH PROPOSALS. SUCH A STUDY MIGHT BE PART OF A LARGER, MUCH-NEEDED EFFORT TO EXAMINE THE PROCESSES OF DECISION-MAKING IN ALLOCATING RESEARCH SUPPORT.

ROLE OF PEER REVIEW IN SUPPORT OF UNORTHODOX INNOVATIVE RESEARCH

References

1. Douglass, C. D., and James, J. C. Support of New Principal Investigators by NIH: 1966 to 1972. Science 181: 241-244 (1973).

COMMUNICATIONS WITH APPLICANT-INVESTIGATORS AND DOCUMENTATION OF REVIEW RESULTS

IMPACT OF INAPPROPRIATE DISCLOSURE OF REVIEW DELIBERATIONS

The dual peer review process, and the decision-making process, have been described in previous sections and will not be restated here. In addition, the impact of the Privacy Act on the peer review process has been discussed in detail. The implications of this Act are that the principal investigator has a right to a complete and detailed explanation of the review and decision concerning his/her application. However, this explanation must be accurate and reflect the final critique as prepared by the Executive Secretary of the particular IRG involved.

Final formal action on an application is taken only after a number of steps in the review and decision-making process have occurred. No one action along this orderly path can be considered binding until the final Institute position has been offically conveyed to the applicant-institution and principal investigator.

Unauthorized premature disclosure of the results of any portion of the total review process may cause serious and unnecessary problems. Thus, a potential grantee cannot assume that an award will or will not be made based on a rumor or on casual comments by persons involved in the review process or by NIH review or administrative (program) staff. Such comments are inappropriate, and if made or if rumors are circulated, applicants do expect certain actions to occur. For this reason, the proceedings of review meetings, the discussions which were held, the preliminary results of a site visit, the written preliminary comments of the assigned reviewers, are all considered as confidential working materials; they must not be discussed openly by those involved in the review or by the staff, with the applicant-investigator, or with any other unauthorized persons.

It is obvious that there are many points during the process at which premature disclosure can occur. For example, if a project site visit is performed as part of a large review, the report which is prepared for submission to a parent initial review group, is clearly preliminary and must be considered a working document. The written comments of assigned reviewers and outside consultants, similarly, are preliminary working documents. After full discussion at an IRG meeting, the essence of the roundtable discussion and the salient features on which recommendations are based, are incorporated into the summary statement prepared by the Executive Secretary. Again, it should be emphasized that the preliminary evaluations may be significantly changed as a result of additional information and discusson during the meeting, and are not necessarily reflected in the final summary statement. Once the relevant portions of the working documents (individual preliminary reviews, site visit reports, etc.) have been used for the preparation of the official summary report, they are of no further use or significance and thus should not be retained.

Furthermore, it often happens that an Advisory Council/Board may recommend, and an Institute may make, an award for a grant which was recommended for approval by the IRG with only moderate enthusiasm but which involves studies within an area of high program interest to the BID. Conversely, a grant application which has been approved with a high priority score may have to wait for funding if other program priorities take precedence and there are limited funds available. Thus, even the summary statements prepared by IRGs do not reflect final BID action but only the IRG recommendation.

It is therefore esstential that confidentiality be maintained by those persons involved in the review and the administrative details of the process so that the applicant-investigator obtains the appropriate and accurate information.

The Study Team recommends that NIH reiterate its policy that all persons involved in grants peer review be constantly alert concerning the need for confidentiality of all aspects of the review, and that there be strict adherence to the policy concerning the prohibition of premature unofficial disclosure of the results of site visits, IRG review, and Council actions.

RELEASE OF SUMMARY STATEMENTS TO PRINCIPAL INVESTIGATORS

NIH takes very seriously its responsibility for informing principal investigators and their institutions about the outcome of grant application review. Over the years, NIH procedures and practices in this area have evolved from (1) routinely providing only the "bottom line" (i.e., "approval," "disapproval," or "deferral") to principal investigators and their institutions to (2) providing principal investigators, upon request, with important additional information, namely, paraphrased versions of the reviewers' critiques of their applications (the "reasons letters"). Because many of the comments and criticisms associated with a grant application pertain directly to the quality of the principal investigator's ideas, training, experiences, and accomplishments, NIH traditionally has viewed the reasons letter as a confidential communication to the investigator and has left to him/her the decision as to whether or not to share the letter with others.

In order to make the best use of the technical merit assessments of grant applications by peer reviewers, NIH staff is considering the feasibility and desirability of adopting additional or alternative approaches for communicating with principal investigators about the results of review. For example, some believe that the reasons letters should be sent out routinely on the initiative of NIH rather than strictly upon request. Others note that paraphrasing the contents of a summary statement produces, at best, only a good approximation of the reviewers' comments and, at worst, an account that is incomplete, inaccurate, or otherwise distorted as compared to the original. Still others point out that the task of preparing reasons letters is a significant component of the workload of BID extramural staff.

The Privacy Act has added a new dimension of concern. As now implemented, the BIDs provide edited summary statements, without priority scores, to principal investigators upon request, even if the request is received before a National Advisory Council/Board has completed action on the application. Therefore, under present NIH policies, an investigator who exercises his/her rights to the limit, may have an almost complete copy of the initial review group summary statement concerning his/her application even before a National Advisory Council/Board has had the opportunity to consider it. At the other extreme, an investigator who takes no initiative beyond submisson of an application may never learn anything more than the final action taken by a National Advisory Council/Board.

Given the importance that NIH attaches to good communications, it does not seem in the interests of either the principal investigators or the NIH to allow the information contained in summary statements to exist only in NIH files because investigators may not understand the law and/or NIH procedures well enough to know how to obtain these detailed critiques of their applications. Because of the foregoing concerns, the Study Team explored the question of whether NIH should go beyond its present practice of informing principal investigators about review results and, if so, what new approaches should be considered.

RECOMMENDATIONS AND DISCUSSION

1. THAT, AS SOON AS PRACTICAL AFTER A BID's NATIONAL ADVISORY COUNCIL/BOARD COMPLETES THE REVIEW OF A GRANT APPLICATION, THE BID SHOULD ROUTINELY SEND THE SUMMARY STATEMENT, WITH THE PRIORITY SCORE DISPLAYED, TO THE PRINCIPAL INVESTIGATOR. BECAUSE SUMMARY STATEMENTS FREQUENTLY CONTAIN FORTHRIGHT AND CRITICAL COMMENTS CONCERNING THE PRINCIPAL INVESTIGATORS, THIS RECOMMENDATION IS MADE CONTINGENT UPON THE UNDERSTANDING THAT THESE DOCUMENTS WILL BE RELEASED BY NIH ONLY TO THESE INDIVIDUALS. IF IT IS DETERMINED THAT SUCH NIH-INITIATED RELEASE OF SUMMARY STATEMENTS COULD REASONABLY LEAD TO A REQUIREMENT THAT NIH MAKE THESE DOCUMENTS AVAILABLE ALSO TO APPLICANT INSTITUTIONS OR OTHERS, THEN THE STUDY TEAM RECOMMENDS THAT SUMMARY STATEMENTS WITH PRIORITY SCORES DISPLAYED BE RELEASED ONLY UNDER PRIVACY ACT PROCEDURES, I.E., UPON RECEIPT OF AN APPROPRIATE REQUEST FROM THE INDIVIDUAL CONCERNED.

   This recommendation was one of the most difficult for the Study Team to formulate and was arrived at only after agonizing deliberation. On the one hand, the Study Team is unanimous in believing that principal investigators deserve to have a detailed account of the results of review in a timely fashion and in a form that is most likely to be a complete and accurate account of the reviewers' findings and recommendations. On the other hand, the Study Team is unanimous in believing that NIH should take no action that would jeopardize or endanger the privacy rights of individuals. The Study Team would be deeply distressed if NIH-initiated release of summary statements to principal investigators somehow were to trigger or catalyze a chain of events (e.g., judicial actions or legislative reinterpretations) that caused the provisions of the Privacy Act no longer to apply to summary statements. Because of this ambivalence the Study Team elected to make its recommendation conditional and urges that NIH seek guidance on this matter at the earliest possible time.

   Carefully formulated as it is, the recommendation still engendered a significant diversity of reactions from Study Team members. Some regarded it as the strongest recommendation possible in the face of uncertainties about judicial and/or legal consequences and therefore endorsed it enthusiastically. Others feared that NIH-initiated mailing of summary statements, or reasons letters, to principal investigators would be interpreted by the scientific community as gratuitous communications or, even worse, as attempts to direct research. Consequently, they preferred the present practice of providing detailed accounts of review results only upon request. This latter group, within the Study Team, believed that the inefficiency associated with having principal investigators explicitly request summary statements or reasons letters is an acceptable price to pay for ensuring that detailed accounts of the results of review go only to those principal investigators who take specific action to obtain them.

Although routine release of summary statements to principal investigators would be an NIH initiative rather than the investigator's, the summary statements would nevertheless be processed consistent with current NIH Privacy Act procedures. This involves having them edited as necessary to ensure that the principal investigator receives only that information which pertains to him/her. While implementation of such a procedure clearly is feasible, it will produce some increase in the workload of BID officials thoughout NIH, especially during the weeks immediately following National Advisory Council/Board meetings.

The biomedical research community is becoming increasingly knowledgeable about the rights of individuals under the Privacy Act, and several BIDs are reporting an increase in the number of requests from principal investigators for the summary statements concerning their applications. Therefore, NIH-initiated release of summary statements is likely only to hasten a growth in workload that already is inevitable and shift it from officials designated to handle Privacy Act procedures to other BID officials.

The use of the summary statement as a vehicle for communication from NIH to principal investigators (whether at NIH's initiative or upon request) may present another problem. For those applications on which a National Advisory Council/Board acts in concurrence with the initial review groups' recommendations (the vast majority of cases), it generally will be sufficient to mail only the summary statements. However, for those applications on which a National Advisory Council/Board takes an action that is different from or in addition to that recommended by an initial review group, the BID would need to include, with the summary statement, an indication of the Council's/Board's decision and the supporting rationale. Case-by-case attention by senior BID staff therefore will be necesary. On the other hand, using summary statements to communicate with principal investigators would reduce the workload of BID extramural staff insofar as the preparation of reasons letters is concerned.

Despite these predictable difficulties associated with implementing its recommendation, the Study Team believes that the advantages of making summary statements readily available to principal investigators (hopefully at NIH's initiative) far outweigh the disadvantages. First, investigators would have more direct knowledge of the thoroughness and even-handedness with which peer reviewers carry out their tasks; this alone could do much to reduce concerns about the effectiveness and equity of this process. Second, principal investigators would have the benefit of valuable advice and consultation from a group of knowledgeable and distinguished experts. Third, the knowledge that summary statements are, ultimately, to be available outside NIH as well as within, would be an incentive for both those who prepare them and those who use them in decision-making to be certain that their contents are accurate, complete, and readable and that references to specific investigators are made with full respect for the dignity of the individuals involved. Fourth, if summary statements were to be released with the priority scores displayed, investigators would have a much clearer picture of how reviewers perceived the relative scientific merit of their applications and would be in a better position to appreciate how program relevance considerations and/or BID staff decisions affect their chances for an award. Fifth, NIH would be responsive to the advice of the scientific and lay public, for the concept of transmitting summary statements to principal investigators either automatically or upon request, received a relatively strong endorsement from both the participants in the NIH grants peer review system (Supplement 1) and some members of the public (both scientists and lay people) who testified at the public hearings.

Although the availability of summary statements, with priority scores displayed, to principal investigators, will provide them with potentially useful information about the scientific merit review of their applications, communications regarding the likelihood of awarding of approved applications are inadequate. It is not generally appreciated that, in addition to the scientific merit evaluation, subsequent decisions concerning award of approved applications are made at the BID level, based on programmatic and/or budgetary considerations. An accurate and clear explanation of these important additional considerations is essential to avoid misunderstandings and unjustified assumptions, on the part of unfunded investigators, regarding the quality of the scientific merit review of their applications; such misunderstandings frequently result in unjustified complaints about the peer review system.

2. THAT, WHEN, FOLLOWING COMPLETION OF THE INITIAL TECHNICAL MERIT REVIEW OF A GRANT APPLICATION BUT PRIOR TO FINAL ACTION BY A NATIONAL ADVISORY COUNCIL/BOARD, A BID COMPLIES WITH A PRINCIPAL INVESTIGATOR'S REQUEST FOR A COPY OF THE PERTINENT SUMMARY STATEMENT (AS PRESENT NIH POLICY FOR IMPLEMENTING THE PRIVACY ACT REQUIRES), THE BID SHOULD PROVIDE THE DOCUMENT WITH PRIORITY SCORE DISPLAYED AND ADVISE THE REQUESTOR THAT THE INFORMATION BEING TRANSMITTED IS INTERIM IN NATURE AND THAT ANY ATTEMPT, AT THIS POINT IN THE REVIEW PROCESS, TO MODIFY THE ORIGINAL APPLICATION OR PROVIDE COMMENTARY FOR CONSIDERATION BY THE COUNCIL/BOARD MAY RESULT IN A DEFERRAL OF THE APPLICATION TO THE NEXT REVIEW CYCLE.

The Study Team believes that, in complying with requests for summary statements under the Privacy Act before completion of the review process, BIDs should follow the same practice as is proposed in Recommendation 1 above, i.e., release the document with the priority score displayed. There seems to be no compelling reason why the priority score should be withheld in one case but made available in another. However, BIDs should make it clear to requestors that they are receiving interim information and that any attempt to influence the outcome of the review process, at this point, could produce significant delay.

3. THAT NIH SHOULD REQUEST AUTHORIZATION, THROUGH EITHER REGULATION OR LEGISLATION, AS APPROPRIATE, TO RELEASE AN INITIAL REVIEW GROUP SUMMARY STATEMENT TO THE PRINCIPAL INVESTIGATOR NAMED IN THE APPLICATION ONLY AFTER THE REVIEW OF HIS/HER GRANT APPLICATION IS COMPLETE, i.e. AFTER REVIEW BY NATIONAL ADVISORY COUNCIL/BOARD.

Under the present interpretation of the Privacy Act (as described above), principal investigators have the opportunity to obtain interim information about the review of their applications and to react to it, e.g., by submitting additional material and/or rebuttal statements. Ignoring the fact that such action most often will produce a delay in the review, the written interchange between a principal investigator and NIH could result in a more accurate assessment of the scientific merit of the application and the investigator's abilities. In fact, several participants in the public hearings stressed the desirability of incorporating a rebuttal step into the grants peer review procedure.

On balance, however, the Study Team believes that the release of interim information on the review of an application creates more problems than it solves. There will be occasions when an investigator challenges the review of an applicat on the BID is prepared to fund anyway, and such reaction to premature information may serve no good purpose. There will be other occasions when an application, judged to be highly meritorious by an initial review group, is not selected for an award by the BID because a National Advisory Council/Board assigned a "low program relevance" rating; raising false hopes in an investigator with interim information only to dash them in a few months seems most unfortunate. The Study Team believes that the public interest would be better served if NIH were allowed to complete its peer review procedures before sending principal investigators an account of the reviewers' findings and recommendations. Moreover, if NIH's policies were changed such that summary statements were sent out automatically at the completion of the review process, the denial of requests for interim documentation would not affect what a requestor receives but only when he/she receives it and thus still would be consistent with the spirit of the Privacy Act.

PRIORITY SCORES ON SUMMARY STATEMENTS

Traditionally, NIH has assigned to each approved grant application, a numerical score signifying its relative scientific merit as perceived by an initial review group. This so-called priority score originally was determined by having each reviewer rate the application on a scale from 1.0 (the highest) to 5.0 (the lowest) in increments of 0.5, averaging the individual ratings, and multipying by 100. The resulting three-digit number came to be called the raw priority score and, for many years, was used uniformly throughout NIH.

But serious doubts about the process eventually developed. For example, a concern arose in several quarters of NIH that, consciously or otherwise, members of initial review groups sometimes might be assigning especially good priority scores to grant applications in certain areas of special interest to them, in the hope of increasing the likelihood that awards would be made. Similarly, some began to believe that reviewers sometimes express their lack of enthusiasm for certain areas of science by assigning comparatively low priority scores to all applications falling within those categories. Obviously, to the extent that review groups behave in these ways, the utility of the raw priority score as a true indicator of relative scientific merit _per se_, would be compromised.

In order to circumvent this, a mathematical procedure was devised to compare and adjust all the scores assigned at a given meeting of an initial review group in concordance with those ratings assigned at the group's two preceding meetings. The purpose of this procedure is to place the applications appraised by any one initial review group on a normal distribution curve. Thus, if an unusual number of applications were to be given scores higher or lower than the "normal" value (about 250), such scores would be redistributed on the curve so that most of them fall in the middle "normal" area where, in theory at least, they should fall. By this procedure, the mean score is adjusted to 250 and the standard deviation to 70. The resultant values are called the "normalized scores." The assumption underlying this statistical procedure is that, on the average, approved applications from the various NIH initial review groups are roughly equivalent in terms of scientific merit.

Currently, there appear on summary statements both the raw priority scores and the normalized ones (if the priority scores of 25 or more applications are available to be included in the normalization procedure). Each awarding unit has the option of using either system but is consistent in this regard. Confusion about the procedure can and does occur, however, particularly on the part of members of National Advisory Councils/Boards. Moreover, the normalization process can change the priority score significantly (on occasion, by 50 points), especially if reviewers consciously attempt to manipulate the procedure. When an adjustment of this magnitude is involved, it could easily mean the difference between award and non-award of a grant depending upon which score is used by the BID in making the awarding decisions. Because of its concerns about these matters, the Study

Team elected to review the present practices involving the raw and normalized priority scores and to attempt to determine whether any changes might be indicated.

RECOMMENDATIONS AND DISCUSSION

1. THAT A "SINGLE PRIORITY SCORE" CONVENTION SHOULD BE ADOPTED FOR USE THROUGHOUT NIH.

   The present practice of having both raw and normalized priority scores on summary statements and, leaving to each BID, the decision as to which convention it will follow, obviously, is viable and could be continued indefinitely, without irreparable harm to the grants peer review system. In the view of some NIH staff, however, it is becoming increasingly difficult to justify to themselves, to the scientific community, and to others interested in NIH's activities, why it is appropriate for something as fundamental as the convention for scoring the perceived scientific merit of a grant application to vary from one BID to another. Recognizing the significance which all BIDs attach to priority scores when they make award selections and other program management decisions, the Study Team believes that a single priority score convention for all of NIH would be preferable to the present practice.

2. THAT, BEFORE ADOPTING A SINGLE PRIORITY SCORE NOTATION SYSTEM FOR USE BY ALL BIDs, THE NIH SHOULD CONDUCT A STUDY OF BID PRACTICES REGARDING THE USE OF THE "RAW" AND "NORMALIZED" PRIORITY SCORES, TO DETERMINE WHETHER THE UNIFORM NIH-WIDE CONVENTION SHOULD BE THE USE OF THE RAW SCORE EXCLUSIVELY OR THE PRESENT NORMALIZED SCORES, WHENEVER THEY ARE AVAILABLE (OR OTHERWISE THE RAW SCORES), OR THE DEVELOPMENT OF A NEW PROCEDURE FOR COMPUTING, REPRESENTING, AND/OR ADJUSTING PRIORITY SCORES TO COMPENSATE FOR DIFFERENCES IN GROUP RATING BEHAVIOR.

   There is considerable difference of opinion among the NIH awarding units as to the relative utilities of the "raw" and "normalized" priority scores in their program management activities. Some BIDs use the raw scores exclusively in the belief that, at least for the initial review groups which serve their programs, the normalization procedure does more to confound the interpretation of scientific merit assessments than to reduce variance in the rating behavior of initial review groups, either between such groups or within the same group over time. Other BIDs take the opposite view. Because the issue is so important and the answer is not obvious, the decision to adopt a single priority score convention should not be taken until there has been a study of the relative strengths and weaknesses of the two alternative practices presently in use. Moreover, the Study Team recognizes that there also are other potentially useful methods for computing and/or mathematically adjusting scientific merit ratings and these also should be studied. For example, it has been suggested that the present three-digit representation for priority scores may imply a greater degree of precision in the initial review groups' ratings than they are able to achieve and that a more coarse representation (with or without mathematical adjustment to compensate for differences in group rating behavior) might be more appropriate.

3. THAT, UNTIL SUCH TIME AS A SINGLE NIH-WIDE PRIORITY SCORE NOTATION SYSTEM IS ADOPTED, ALL COPIES OF SUMMARY STATEMENTS WHICH A GIVEN BID SENDS TO PRINCIPAL INVESTIGATORS SHOULD DISPLAY EITHER THE RAW PRIORITY SCORE OR THE NORMALIZED PRIORITY SCORE (IF AVAILABLE) BUT NOT BOTH, DEPENDING UPON WHICH OF THE TWO CONVENTIONS THE PARTICULAR BID FOLLOWS.

The display of dual priority scores on the copies of summary statements which are released to principal investigators would inevitably be a continuing source of difficulty for BID staff and others as they attempt to advise principal investigators about the outcome of the review process and their prospects for receiving an award. This difficulty can be avoided easily by instructing the staff of each BID to delete from the copies of summary statements which are to be sent to principal investigators the priority score that the BID does not use in its decision making. This special editing step would be needed of course, only until the NIH adopts a single priority score system and the printed summary statements then have only one score displayed.

4. THAT, NIH SHOULD CONDUCT STUDIES OF:

   a. VARIATIONS IN INDIVIDUAL REVIEWER AND REVIEW GROUP BEHAVIOR IN RATING APPLICATIONS, OVER TIME, AND AMONG DIFFERENT IRGs, AND OF THE FACTORS WHICH ACT TO INCREASE OR DECREASE SUCH VARIABILITY;

   b. VARIATIONS IN THE QUALITY OF GRANT APPLICATIONS ASSIGNED TO A GIVEN IRG FROM ONE REVIEW ROUND TO THE NEXT, OVER TIME; AND VARIATIONS IN THE QUALITY OF GRANT APPLICATIONS ASSIGNED TO DIFFERENT IRGs;

   c. THE EFFECTS ON THE REVIEW PROCESS OF DISPLAYING THE RAW PRIORITY SCORE TO THE INITIAL REVIEW GROUP MEMBERS IMMEDIATELY AFTER THEY ASSIGN THEIR INDIVIDUAL SCIENTIFIC MERIT RATINGS, AND OF GIVING THEM THE OPTION, AT THAT POINT, TO REOPEN DISCUSSON AND RERATE THE APPLICATION.

The normalizing computation used by NIH is an indirect procedure for compensating for variability in IRG rating behavior over time. Studies a. and b. above would attempt to determine whether there are more direct approaches, such as the specific procedure suggested for trial in c., which might be used in conjunction with, or as a replacement for, the normalizing procedure to assure consistent, objective IRG ratings over time. Study b. would be a study to determine the extent to which differences among IRGs or within an

IRG overtime, in the average priority score and approval/disapproval ratio, reflect real differences in the quality of the applications under review rather than arbitrary shifts in group rating behavior. For study b., retrospective studies using citation methodology or the like, might be appropriate.

Under present initial review group procedures, each member ultimately expresses his/her view of the scientific merit of a grant application which is being recommended for approval by privately assigning a numeric rating. These individual ratings are collected by the executive secretary and his/her staff and averaged. Because the averaging of individual reviewers' ratings for any given application generally is not performed until well after the completion of the group's deliberations (often several days after the meeting), reviewers have no automatic feedback as to either what specific rating each member assigned or what the group's aggregate rating was.

There are several problems inherent in this procedure that, in the opinion of the Study Team, make a special study--and some experimentation with alternative approaches--seem warranted. First, because the group has only the primary and secondary reviewers' written comments and the subsequent oral exchange of opinions available at the time of rating, the members may not be fully aware of the nature or extent of differences of opinion which may exist within the group; and the Chairman may inadvertently terminate discussion when, in fact, further deliberation might be likely to produce a greater consensus. Second, without access to the resultant raw priority score, immediately following its discussion and rating of an application, the group has no opportunity to satisfy itself that the score is truly consistent with the spirit of its discussions and, if not, to reopen deliberations and possibly rerate the application. Under the present procedure it sometimes happens that the specific written and oral comments from which the executive secretary prepares the summary statement for an application are appreciably at variance (either more or less favorable in tone) with the implications of the priority score actually assigned.

There are a variety of techniques to effect quick feedback within initial review groups about individual reviewers' ratings and/or the aggregate score. These techniques range from the display of a hand-held score by each reviewer (as in done by judges of diving competitions) to the use of sophisticated electronic systems for instantaneous recording of individual ratings, computations of the aggregate score, and display of results. The Study Team recommends that NIH conduct a study comparing the present "no feedback" procedure with one or more alternatives involving immediate feedback about individual and/or group rating behavior. Such a study should include mechanisms to preserve the privacy of individual ratings by IRG members and should not be construed as a means to pressure IRG members to change their ratings. Rather, the study should be considered as a further means of studying rating behavior and an effort to develop an improved rating procedure.

GRANTS PEER REVIEW APPEALS SYSTEM

GRANTS PEER REVIEW APPEALS SYSTEM

In his April 28, 1975, memorandum establishing the NIH Grants Peer Review Study Team (GPRST), the Acting Director, NIH, asked the Study Team to address "the entire process of peer review," and to give specific attention to..."the need, advantages, and disadvantages for a technical merit review 'appeal' mechanism for applicants and an assessment of the process currently in use..." This suggestion led to formation of a Subcommittee on Appeals early in the activities of the GPRST. In addition to those members chosen from the Study Team, the Subcommittee was supplemented by additional members representing various functions in the grants peer review process in different BIDs. The substance of the report of the Subcommittee was adopted by the Grants Peer Review Study Team.

I. BACKGROUND

Several factors have led to concerns about an NIH grants peer review appeals process, most notably, perhaps, a portion of the Report of the House of Representatives Committee on Interstate and Foreign Commerce on the National Cancer Amendments of 1974. In that portion of the Report dealing with Peer Review of Grant Applications and Contract Projects, the Committee expressed the hope that NIH would take steps to ensure "...a just and proper recourse...to every unsuccessful applicant who wishes to appeal an adverse decision," and also voiced the feeling "...that, if the present appeals mechanisms are determined to be insufficient, it would be appropriate to consider the creation of an independent appeals process..." (1).

Other action in the 94th Congress focused more specifically on an appeals system for the National Science Foundation. Senate Bill S. 2427 and House Bill H. R. 9492 both suggested that NSF "...establish provisions for appellate procedures to independently review... proposals disapproved by the Foundation..." (Appendix I-1). While these two bills failed to be enacted, they presumably had some effect on the NSF directorate, which issued, in January 1976 an Important Notice on Reconsideration of Proposals Declined by NSF (Appendix I-2). This Notice aims to standardize the agency's methods for reconsideration of proposals which have been "declined," a term signifying that NSF will not provide grant support in response to an application.

Most specific to NIH and the Grants Peer Review Study Team tasks, a number of inputs to the GPRST have related to the need for an improved NIH grants review appeals system. Such opinions have been expressed in the three hearings held by the GPRST, have been received in a significant number of letters from the biomedical research community, and have appeared in comments on the questionnaire from members of NIH peer review committees. All of these comments focus on two broad aspects of an appeals system:

- The need to establish a system whereby an applicant may challenge the assignment of an application to an initial review group (IRG) for evaluation of scientific merit, and to a Bureau, Institute, or Division (BID) for funding considerations. Applicants have been concerned regarding difficulties with what they perceive as inappropriate assignments to IRGs which would allow an application to be evaluated by reviewers who may not be the best qualified to do so, or whose members might have potential biases in relation to the applicant institution, the principal investigator or staff, or the suggested project. Applicants sometimes also question the assignment to an awarding unit, or BID, which may have less potential interest in the proposed project than another BID.

- The need for a system which will enable an applicant-investigator to receive the total information which was utilized in making final recommendations on the application, and to have the opportunity to challenge or rebut such recommendations. Applicant-investigators sometimes perceive factual errors in review, e.g., data or information overlooked or not considered in the review, or data or statements misinterpreted by review groups. Or they may question the validity of judgments regarding the scientific evaluation, budget adjustments, or ethical or biohazard considerations of proposed projects.

A. Current Appeals Mechanisms (Appendix I-3)

The present NIH grants peer review system includes long-established and well-tested mechanisms for handling these processes regarding which suggestions have been made for improved handling of disputes or appeals. These involve the referral process, and the processes for communications regarding the outcome of scientific reviews, described elsewhere in this report.

1. Referral — The receipt and assignment of applications involve several steps which are crucial and have important implications for the application as it moves through the peer review system. These functions are responsibilities of the Referral Branch in the Division of Research Grants (DRG), whose professional staff is selected on the basis of varied and specialized backgrounds suitable for determining the competencies of IRGs to review various applications, and for identifying appropriate granting components in light of their established areas of program interest. The primary consideration for IRG assignment is that the scientific methodology described in the application is best evaluated by eminently qualified scientists who, themselves, use or are familiar with the same methodologies. Assignment to a BID derives from an overall evaluation of the aims stated in the application and the investigator's statement of the significance of the proposed work. Actual assignments are based primarily on guidelines developed by DRG in collaboration with the BIDs.

2. Communications with Applicant-investigators

Summary statements relating the essential elements of a review group's considerations and recommendations are prepared by the executive secretary of that particular review group. Each summary statement follows a standard format in presenting a set of information points describing the application and explaining and justifying the review group's recommendations regarding the pertinent application and includes a priority score which expresses, in numerical terms, the group's assessment of the merits of the application. Summary statements from IRGs are presented to the National Advisory Councils for the final actions. In past years, investigators requesting the results of their reviews have been provided with abstracts of the summary statements, usually by the BID program staff responsible for the particular applications and projects involved. In more recent months, elements of the Privacy Act of 1974 have led to the release of summary statements to those investigators who request them.

B. Issues

Certain questions and problems have arisen in regard to both the referral process and communications about summary statements, which have led to detailed consideration by the GPRST:

1. Referral Process

a. How can applicant-investigators have an input into the referral process, in order to influence this process in a manner which would allow the appropriate decisions to be made by NIH, but which would take into consideration concerns of investigators that their applications be reviewed by groups most appropriate and competent to do so?

b. How can they express their concerns regarding certain groups or individual advisors responsible for such reviews, who, it is felt, are biased against the investigator or against the types of work or views suggested in the application?

c. How can investigators express their preferences that certain BIDs and corresponding National Advisory Councils consider their applications?

2. Communications

a. What information should be released regarding the outcome of review groups' deliberations? Should priority scores be included?

b. How should this information be communicated, i.e., by informal, oral communications, or by complete, written critiques?

c. To whom should the information be released, i.e., to a principal investigator only, or to institution officials, or co-investigators?

d. When should such information be released, i.e., immediately following an IRG meeting, or following a Council meeting?

e. How can investigators most expediently use whatever information is released, in formulating and appealing elements they feel have "gone wrong" in review of their applications?

3. General Issues

a. What elements of the review process should be made subject to appeal?

b. What recourse may be had to NIH offices outside those which are currently responsible for actions or decisions which rightly are subjects for appeal?

Approaches to some of these questions are covered in other portions of the GPRST Report. Aspects of these questions involving disputes, challenges, or other controversies regarding NIH peer review processes are considered in this section.

II. APPROACHES TO AN NIH GRANTS PEER REVIEW APPEALS SYSTEM

A. Current Elements of an Appeals System

It must be recognized that current NIH policies and procedures have been designed to handle certain aspects of the issues listed above. With regard to BID or IRG assignment, for instance, investigators may, and often do, suggest that applications be assigned to certain review groups or to certain BIDs. They may challenge the competence of the IRGs to which applications have been assigned, or may question the ability of specific members of assigned review groups to give recommendations without certain biases or prejudices against the applicant institution, the investigator, or the ideas suggested in the application. Such communications are usually received by and handled within DRG, which is responsible for the assignment procedures, and sometimes involve discussion between DRG and the BIDs.

Other procedures are available to and are practiced by investigators whose applications are not funded due to the grant peer review groups' recommendation either for disapproval or for approval at priority scores which do not permit funding. After receiving word of non-funding of an application and of the reasons leading to such a decision, the investigator may communicate further with the BID which is responsible for both the funding decision, and, at least, the initial communications to the applicant regarding that decision. In return, the investigator, but not the applicant-institution, may learn more detailed reasons for the recommendation regarding the application; such information is based on the summary statement, and may be excerpted from it; or the entire critique-text may be sent to the investigator. While such communications are conducted initially and primarily with the responsible BID, the investigator occasionally turns to the executive secretary of the IRG responsible for the scientific review, to learn more regarding the scientific rationale for the recommendation.

Furnished with these comments about the previous scientific reviews, the investigator is then free to submit a revised application, incorporating as many changes, as deemed desirable to meet the critical objections noted in regard to the original application. Over the past few years, such revised applications have constituted approximately ten percent of the review workload.

B. New Elements for a Formalized Grants Review Appeals System

While designed to handle most disputes arising in connection with the peer review of grant applications, the procedures now in force have certain deficiencies. Basic to most of these is the fact that the mechanisms for challenge or appeal are informal, handled on a person-to-person, ad hoc basis, without firmly established NIH policy. This, in turn, leads to a corollary difficulty in that the procedures are not widely known, even though NIH has taken some steps to provide accessibility of the revised application process to disappointed applicants. Moreover, the fact that different kinds of disputes are handled more or less entirely within the same NIH component, or even the same office, originally responsible for the assignement or review procedure, has led many investigators to feel frustrated in their attempts to seek relief from outside that component or office.

RECOMMENDATION 1:

THAT A FORMALIZED NIH GRANTS PEER REVIEW APPEALS SYSTEM BE ESTABLISHED TO CORRECT OR ELIMINATE THE DEFICIENCIES DESCRIBED ABOVE.

This system should have two phases to take into consideration those situations in which the assignment to a BID or an IRG may be challenged, and those in which elements of the scientific review may be challenged. In each case, the appeal must be made by the applicant-institution (or individual) formally applying for the grant, although the principal investigator may be responsible for informal communications regarding scientific aspects of the application. While the appeal or dispute must be received first by the office responsible for previous actions, the new appeals processes, in both cases, require that other officials and offices soon enter into the consideration and decision regarding the arguments proposed by the applicant and the investigator.

RECOMMENDATION 2:

THAT, IN ADDITION TO IMPROVEMENTS IN CURRENT PROCESSES FOR RECONSIDERATION OF ASSIGNMENTS OR REVIEWS, CERTAIN ENTIRELY NEW MECHANISMS BE ESTABLISHED FOR BOTH TYPES OF APPEAL TO HANDLE SITUATIONS WHICH DEMAND ACTION OR DECISION OUTSIDE THE ORIGINAL NIH COMPONENTS AND AT A HIGHER LEVEL.

RECOMMENDATION 2a:

...THAT, CENTRAL TO THESE NEW MECHANISMS BE THE ESTABLISHMENT OF THE POSITION OF OMBUDSMAN APPOINTED BY THE DIRECTOR, NIH.

Many of the responsibilities suggested below for receipt, processing, coordination, and documentation of decisions regarding appeals of disputed applications, should be assigned to the "Ombudsman." To be effective, such a person must be highly experienced with the grants peer review system but administratively independent of DRG and of the BIDs that actively operate the system.

While it is felt that it might be most expedient to handle appeals entirely within DRG, at least for certain types of appeals, it is recommended that resolution involving DRG and applicants should require a detached third party. The Ombudsman should be located in the Office of Extramural Research and Training (OERT), NIH, and should have the responsibility and authority to coordinate the processing of grant peer review appeals at higher levels.

RECOMMENDATION 2b:

...THAT, TO PROVIDE THE NEEDED HIGH LEVELS OF REVIEW RELATED TO APPEALS, THERE BE ESTABLISHED:

a. A GRANTS PEER REVIEW APPEALS BOARD (GPRAB), A PERMANENT COMMITTEE, THE MEMBERS OF WHICH WOULD ACT IN MATTERS CONCERNING ASSIGNMENT OF GRANT APPLICATIONS AND THOSE INVOLVING RECONSIDERATION OF SCIENTIFIC REVIEW OF APPLICATIONS (Table 1).

It should be chaired by the Ombudsman, should include two other permanent members from the Office of the Director (OD), and should include also three members of the Executive Committee for Extramural Affairs (ECEA) from separate BIDs, who would serve rotating terms which would expire in alternate years. An Executive Secretary should also be appointed.

b. GRANTS PEER REVIEW APPEALS PANELS (GPRAP), TEMPORARY GROUPS WHICH WOULD BE FORMED TO ACT IN THE FINAL STAGES OF DISPUTES CONCERNING THE SCIENTIFIC REVIEW OF GRANT APPLICATIONS (Table 1).

Each Panel should be selected by the Ombudsman, who should chair the group. Other members should include two from the GPRAB, of whom one would be from OD and the other from a BID, and two additional extramural staff from other BIDs. All BID members on a Panel should be from those BIDs not involved in the application dispute being considered. Additional scientific, administrative, and public members should serve on each Panel as required. An Executive Secretary should document the proceedings of each Panel.

III. CRITERIA FOR RECONSIDERATION OF NIH ACTION ON GRANT APPLICATIONS

RECOMMENDATION 3:

THAT THE FOLLOWING CRITERIA BE ESTABLISHED FOR RECONSIDERATION (APPEAL) OF NIH ACTION ON GRANT APPLICATIONS:

A. Who may initiate an appeal?

Only the applicant may initiate an appeal or request reconsideration of an application. The applicant is understood as the institution (or individual) formally applying for a grant award. The Principal Investigator (PI) will normally be the scientific representative of an applicant-institution and may be responsible for informal communications involving scientific aspects of that particular application, but to initiate a formal appeal, he/she must obtain the written endorsement of the applicant-institution.

B. What may be appealed?

An applicant may feel that it has reason to challenge or appeal either or both of the following situations during the peer review cycle:

1. Assignment of the application to an IRG or BID may be challenged and appealed after notification of the assignment. Prompt action by the applicant-institution should be encouraged in such cases, so that further consideration of the assignment may be accomplished in

the same review cycle. If an assignment is appealed after review, the applicant must explain and justify why the appeal was not entered earlier.

2. After receipt of the summarized comments pertaining to evaluation of the application, an applicant-institution may appeal the evaluation of the application if there is evidence that invalid judgements, inadequate review, or factual or administrative errors occurred in the scientific review. Such appeals may be made in any situation where the application does not result in a grant award; National Advisory Council or Board program or policy recommendations may not be challenged, however.

It is felt that, to a considerable degree, the proposed mechanisms, described below, for challenging or appealing assignments or reviews will accommodate situations covering appeals and disputes, before and after IRG review, and for all unfunded applications, whether favorably or unfavorably recommended. It is also recognized that instructions for applicants must make clear that any appeal may result in delays such that the application may be held until the succeeding Council round. Upon initiation of any appeal action, the pertinent application would be withdrawn from any further review or processing action pending resolution of that appeal. A similar delay would apply to any amended application, resubmitted while appeal of the original application is being considered. Further, appeal of any portion of a multi-project application e.g., program project or center grant, would require that the entire application be considered under appeal.

IV. MECHANISMS AND PROCEDURES FOR APPEALS CONCERNING ASSIGNMENT OF GRANT APPLICATIONS

RECOMMENDATION 4:

THAT MECHANISMS AND PROCEDURES BE ESTABLISHED FOR APPEALS CONCERNING ASSIGNMENT OF GRANT APPLICATIONS.

If not satisfied, upon notification of an assignment to an IRG or BID, the applicant-institution must proceed as outlined below (with number and letter references to the corresponding lines and columns in Figure 1):

An application (Line 1A) submitted to the DRG Referral Office (1B), is assigned to an appropriate IRG and BID (2B). The applicant-institution or its scientific representative, the PI, receives notification of and considers the assignment (3A) in light of his/her perceptions of the review process.

If dissatisfied, the applicant may request (4A) that the application be assigned to a different BID or IRG, or that specific reviewer(s) be excluded from the review, reasons for which must be detailed. Upon receipt of such a request (4B), the Referral Office decides (5B) on its validity. This appeal is communicated to the Associate Director for Scientific Review, DRG (6B), for his review and concurrence (7B). This is the first step at which the assignment is considered at a level above that where the decision was made originally. Upon receiving a communication about the DRG decision, the applicant considers the action (8A) and decides either to accept the decision (9A) or to submit a further appeal (10A).

If the applicant wishes to continue the appeal, it (including justification) is sent to the NIH Ombudsman (10C). The Ombudsman will form a group (11C) consisting of himself/herself plus two members of the Grants Peer Review Appeals Board (Table 2) who are from NIH components not involved in the appeal(12C). This group constitutes the next higher level of appeal regarding assignments. These three individuals will consult (13C) with the Executive Secretary of the involved IRG (14C), the staff of the BID involved (15C), and the Referral Office (16C) but then will reach an independent decision on the appropriate assignment (17C).

If this decision is to uphold the original assignment (18C), this is communicated to the applicant institution. There is no further recourse (22A). If, however, it is decided to change the assignment (19C) this decision is communicated to the applicant and to the Referral Office, DRG (20B) which will process the application as required by that decision (21B). The applicant will have no further recourse in regard to this revised assignment (22A).

V. PROPOSED MECHANISMS AND PROCEDURES FOR APPEALS CONCERNING SCIENTIFIC REVIEW OF GRANT APPLICATIONS

RECOMMENDATION 5:

THAT MECHANISMS AND PROCEDURES BE ESTABLISHED FOR APPEALS CONCERNING SCIENTIFIC REVIEW OF GRANT APPLICATIONS.

An appeal concerning the scientific review of a grant application will begin only after the PI receives the summarized comments of the review regarding the application as presented in the summary statement. Upon consideration of these comments the PI has three options: (1) to accept the summarized comments without further action; (2) to revise the application in response to the summarized comments and proceed as described below under A., "Resubmission for Review"; or (3) to invoke the appeals process as described below under B., "Appeal of the Scientific Review." As illustrated in Figure 2, appeal of the scientific review of a grant application starts with issuance by the BID of the summarized comments of the scientific review (1B). The PI considers these review comments (2A) and may decide, as one option, to take no further action (3A) or, as the other option, to revise and resubmit the application (4A).

A. Resubmission for Review

This form of appeal has always been available to the applicant and currently is used to a significant extent.

If the application is to be resubmitted, it should be revised in response to the summarized comments by the addition of new information or other amendments. Whatever modifications are made to the application must be indicated in a separate covering letter accompanying the resubmission. This will help reviewers in their reevaluation of the proposal and will also help prevent such material from being overlooked. Failure to point out these modifications could result in the application being returned without further review. Suggestions regarding IRG referral or use of specific reviewers may accompany the resubmitted application.

All this material and information is submitted to the Referral Office, DRG (4B) which will consider the documents and suggestions and assign the revised application to an appropriate IRG (5B). The latter will review the application during the next appropriate review cycle (6B), and its recommendations will be considered by the appropriate BID Council/Board (7B). If the applicant-institution, meanwhile, considers either of these two assignments inappropriate, a new assignment may be requested through the mechanism described above in Section IV.

B. Appeal of the Scientific Review

If the applicant-institution believes there were errors in the original review, an appeal of the evaluation may be made by requesting reconsideration of the review, with rebuttal of the comments (8A). Such a request should be sent to the BID which issued the results of the previous review (8B) and should give the reasons for the request for reconsideration, but submit no modifications to the application. The applicant should submit only such information as is appropriate to support this request for rereview, and must initiate the request within 90 days after the mailing date of the summarized comments, and, in any event, not later than 120 days after the Council/Board meeting.

The BID staff will consider the validity of this request for reconsideration (9B) and, in this process, will consult with the appropriate staff of DRG and the IRG responsible for the initial scientific review (10B). The BID may accept the validity of the request (12B) and may therefore return the materials to the IRG for rereview in the next cycle (13B) or to the BID Council/Board for its consideration (15B).

If however the BID staff considers the request for reconsideration invalid (11B), this decision will be communicated to the institution (12A), which may decide either to accept (14A) or appeal this decision (16A). Such an appeal should be directd to the NIH Ombudsman (16C), giving reasons for the request, but again submitting no modifications to the application, as mentioned above. The applicant should submit only such information as is appropriate to support this request for re-review, and must initiate this request within 20 working days after the mailing date of the BID communication denying the request for reconsideration (11B).

The Ombudsman will consult with two members of the GPRAB (17C), from NIH components uninvolved in the appeal (18C). This ad hoc group may decide to reject the request as one not falling within the jurisdiction of the peer review appeals system (19C), or to consider the request further through formation of a Grants Review Appeals Panel (20C) (see also Table 1). The Panel may decide (21C) to reject the institution's appeal (22C). Neither the ad hoc group's rejection (19C) nor the GPRAP's rejection (22C) may be appealed further by the applicant (21/22A). The Panel may decide, however, that the application deserves further consideration by an IRG (23B) or by an appropriate Council/Board (24B) and may so act. In doing so, the Panel should have authority to decide to what extent the application was adversely affected in the review and to direct the IRG or Council/Board to reconsider previous actions. The NIH appeal process ends with the decision of this Panel, which is reported to the applicant, the BID, and to DRG.

CONCLUSION

The Grants Peer Review Study Team believes that these recommended mechanisms involve both improvements on current procedures and suggestions for new ones. These principles for an Appeals System will furnish the basis for new and improved NIH policy and procedures to handle disputes and should, in turn, help greatly towards better understanding by and equity for members of the broad biomedical scientific community.

TABLE 1

GRANTS PEER REVIEW APPEAL GROUPS

Grants Peer Review Appeals Board - GPRAB

Appointed by Associate Director for Extramural Research and Training (ADERT), NIH

Members: 1 - OERT - Ombudsman - chair
2 - OD - permanent
3 - ECEA (BIDs) - rotate/alternate

Executive Secretary - appointed by ADERT

Grants Peer Review Appeals Panel - GPRAP

Selected by Ombudsman

Members: 1 - Ombudsman - chair
2 - GPRAB (1 OD + 1 BID, from uninvolved BIDs)
2 - NIH extramural staff (*ad hoc*)
+ Additional scientific/adminiistative/public consultants, as required

Executive Secretary - selected by Ombudsman

FIGURE 1

ACTIONS TO RECONSIDER ASSIGNMENT OF GRANT APPLICATIONS TO IRG OR BID

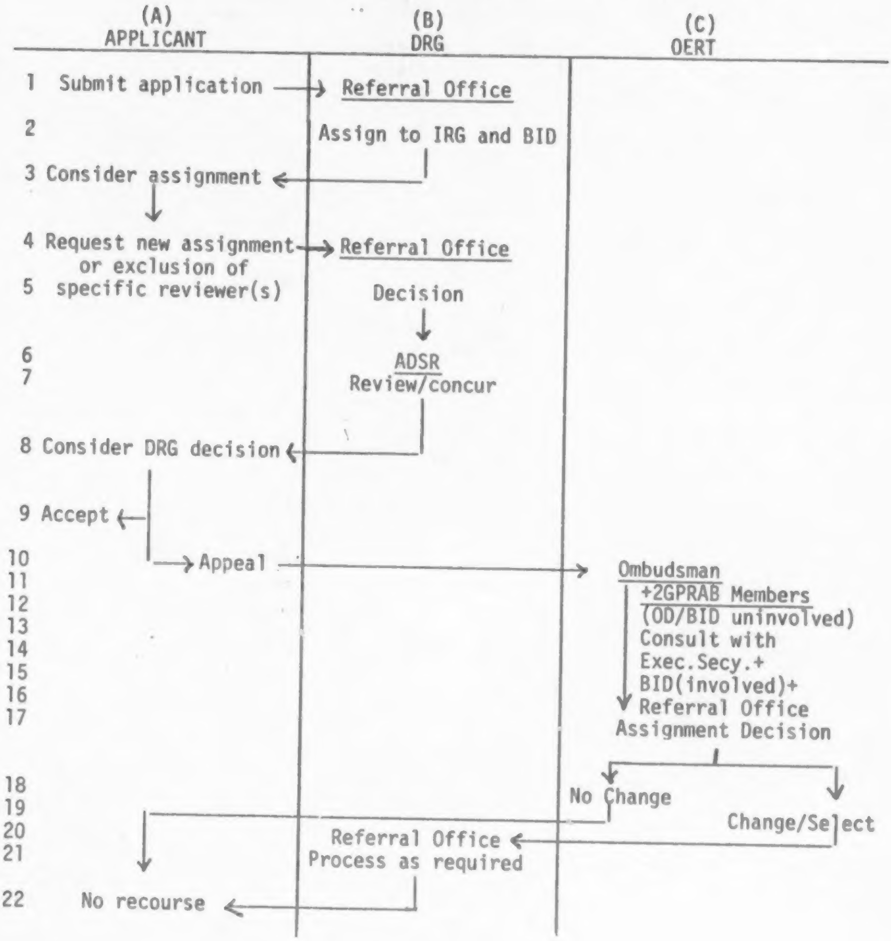
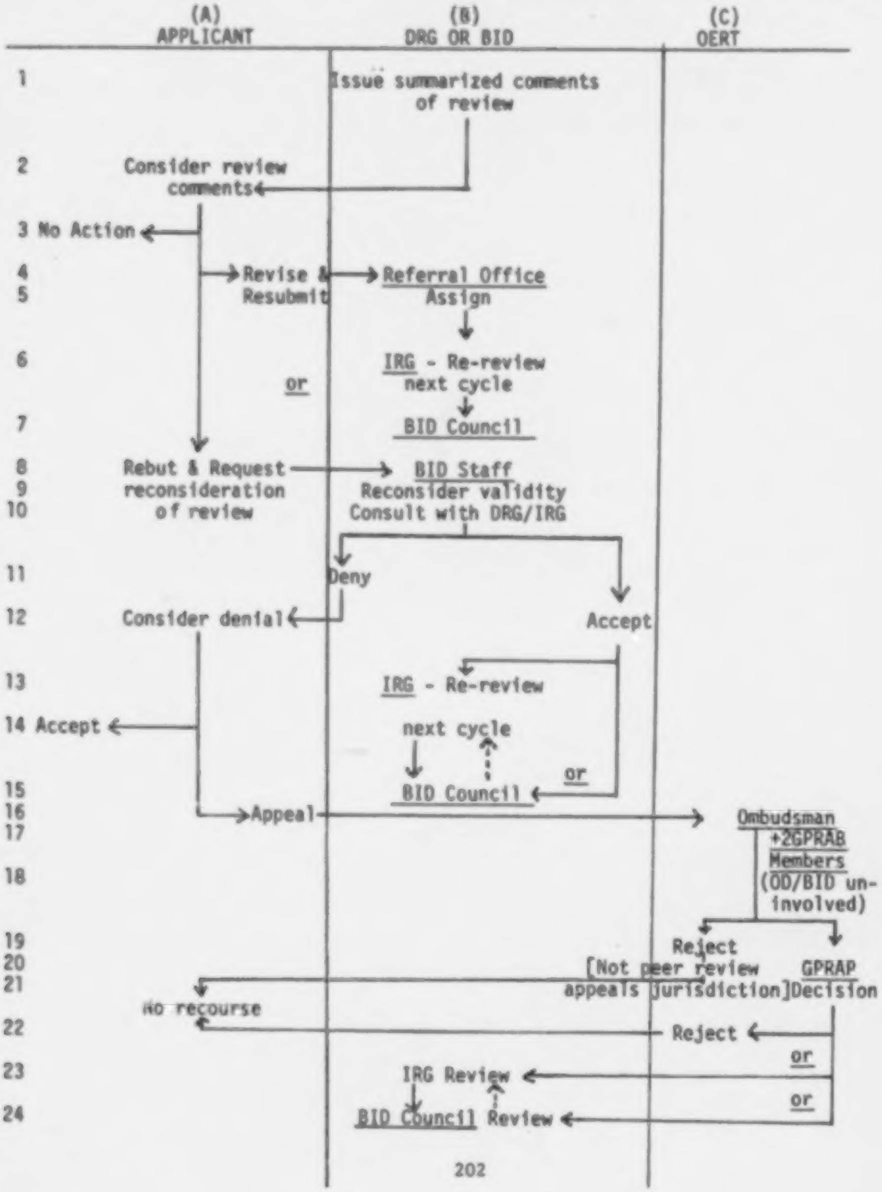

(A)
APPLICANT
(B)
DRG
(C)
OERT
1
Submit application
Referral Office
2
Assign to IRG and BID
3
Consider assignment
4
Request new assignment or exclusion of specific reviewer(s)
5
Referral Office
Decision
6
7
ADSR
Review/concur
8
Consider DRG decision
9
Accept
10
11
12
13
14
15
16
17
Appeal
Ombudsman
+2GPRAB Members
(OD/BID uninvolved)
Consult with
Exec.Secy.+
BID(involved)+
Referral Office
Assignment Decision
18
19
20
21
No Change
Change/Select
Referral Office
Process as required
22
No recourse

FIGURE 2

ACTIONS TO RECONSIDER OR APPEAL SCIENTIFIC REVIEW OF GRANT APPLICATIONS


(A) APPLICANT
(B) DRG OR BID
(C) OERT
1
Issue summarized comments of review
2
Consider review comments
3 No Action
4
5
Revise & Resubmit
Referral Office Assign
6
IRG - Re-review next cycle
or
7
BID Council
8
9
10
Rebut & Request reconsideration of review
BID Staff Reconsider validity Consult with DRG/IRG
11
Deny
12
Consider denial
Accept
13
IRG - Re-review
14 Accept
next cycle
or
15
BID Council
16
17
Appeal
Ombudsman +2GPRAB Members
18
(OD/BID uninvolved)
19
20
21
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Reject
or
23
IRG Review
or
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BID Council Review

ABBREVIATIONS USED

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AAMC - Association of American Medical Colleges

ADAMHA - Alcohol, Drug Abuse, and Mental Health Administration

ADERT - Associate Director for Extramural Research and Training

ADSR - Associate Director for Scientific Review

AIBS - American Institute of Biological Sciences

ARA - Awaiting receipt of application (a file)

ARC - Availability Request Clearance

ASH - Assistant Secretary for Health (in DHEW)

BHM - Bureau of Health Manpower

BID - Bureaus, Institutes, Divisions

BMC - Business Management Consultant

CCC - Comprehensive Cancer Centers

C.F.R. - Code of Federal Regulations

CMO - Committee Management Office(r)

CSRS - Cooperative State Research Service (USDA)

CV - *Curriculum vitae*

DBER - Division of Biomedical and Environmental Research (ERDA)

DCG - Division of Contracts and Grants

D.D.C. - District Court of the District of Columbia

DHEW - Department of Health, Education, and Welfare

DMP - Division of Management Policy

DRG - Division of Research Grants

DRR - Division of Research Resources

ECEA - Executive Committee for Extramural Activities

ERDA - Energy Research and Development Administration

ESRAC - Executive Secretaries' Review Activities Committee

FACA - Federal Advisory Committee Act

FOIA - Freedom of Information Act

F.R. - Federal Register

F. Supp. - Federal Supplement

FY - Fiscal Year

GMAC - Grants Management Advisory Committee

GPRAB - Grants Peer Review Appeals Board*

GPRAP - Grants Peer Review Advisory Panel*

GPRST - Grants Peer Review Study Team

HEW - Department of Health, Education, and Welfare

HEW-474 - Confidential Statement of Employment and Financial Interests

HEW-532 - Request for Approval of Nominees for Public Advisory Committees

HEW NIH OD 0300 - Designation for the official NIH files concerning grants, fellowships, and certain other types of awards

H.R.000 - House of Representatives Bill

H. Rep. - House Report

IMPAC - Information for Management, Planning, Analysis, and Coordination; the computer-based record-keeping system of DRG

IRG - Initial Review Group

MEDLAR - Medical Literature Analysis and Retrieval System (of National Library of Medicine)

MEDLINE - MEDLARS on-line service

MRC - Medical Research Council

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*Projected

NAC/B - National Advisory Council/Board

NAS - National Academy of Sciences

NASA - National Aeronautics and Space Administration

NCI - National Cancer Institute

NEI - National Eye Institute

NHLBI - National Heart, Lung, and Blood Institute

NIA - National Institute on Aging

NIAID - National Institute of Allergy and Infectious Diseases

NIAMDD - National Institute of Arthritis, Metabolism, and Digestive Diseases

NICHD - National Institute of Child Health and Human Development

NIDR - National Institute of Dental Research

NIEHS - National Institute of Environmental Health Sciences

NIGMS - National Institute of General Medical Sciences

NIH - National Institutes of Health

NIMH - National Institute of Mental Health

NINCDS - National Institute of Neurological and Communicative Disorders and Stroke

NLM - National Library of Medicine

NSF - National Science Foundation

OCR - Office of Collaborative Research

OD - Office of the Director, NIH

OERT - Office of Extramural Research and Training

OGC - Office of General Counsel

OLS - Office of Life Sciences (NASA)

ONR - Office of Naval Research

OPPE - Office of Program Planning and Evaluation
PA - Privacy Act
PHS - Public Health Service
PI - Principal Investigator
P.L. - Public Law
PSAC - President's Science Advisory Committee
PSV - Project Site Visit
RB - Referral Branch
RFA - Request for Application
RFP - Request for Proposal
R 01 - Investigator-initiated research grant application
SAB - Statistics and Analysis Branch
SBMP - Subcommittee on Business Management Practices
SCOR - Specialized Centers of Research
S. 000 - Senate Bill
S. Rep. - Senate Report
SIRG - Special Initial Review Groups
SRB - Scientific Review Branch of the Division of Research Grants
SS - Study Section
SSS - Special Study Section
U.S.C. - United States Code
USDA - United States Department of Agriculture
VA - Veterans Administration

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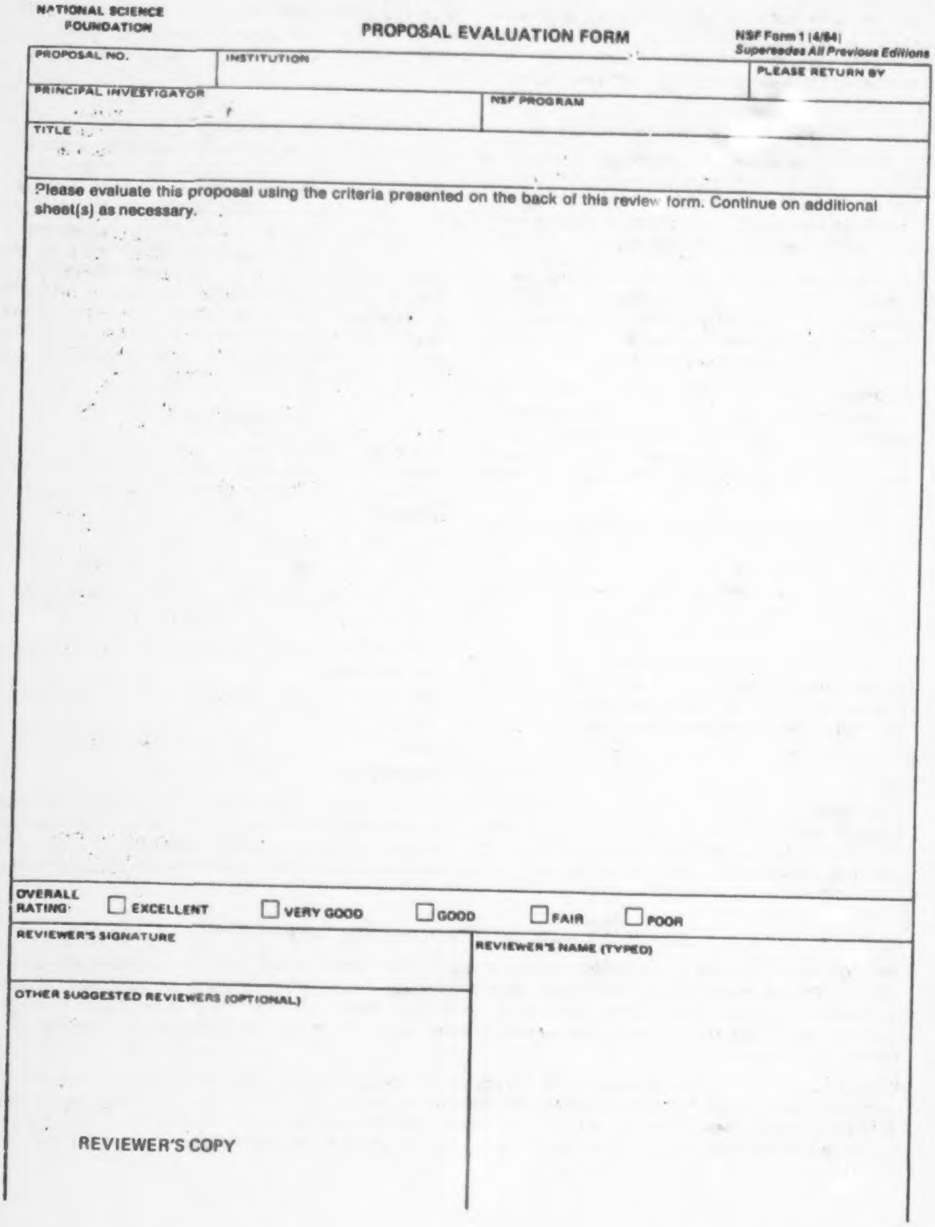
☆ U. S. GOVERNMENT PRINTING OFFICE : 1977 241-161/3032

APPENDIX 2—ADDITIONAL MATERIALS SUBMITTED FOR THE RECORD BY DR. MARY E. CLUTTER



NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1 (4/84)
Supersedes All Previous Editions

PROPOSAL NO.

INSTITUTION

PLEASE RETURN BY

PRINCIPAL INVESTIGATOR

NSF PROGRAM

TITLE

Please evaluate this proposal using the criteria presented on the back of this review form. Continue on additional sheet(s) as necessary.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

REVIEWER'S SIGNATURE

REVIEWER'S NAME (TYPED)

OTHER SUGGESTED REVIEWERS (OPTIONAL)

REVIEWER'S COPY

NATIONAL SCIENCE FOUNDATION

INFORMATION FOR REVIEWERS

In meeting its statutory responsibilities, the National Science Foundation seeks to support the most meritorious research, whether basic or applied. Mail reviews play a key role in the National Science Foundation's evaluation of the merit of research proposals. Please provide both written comments and a summary rating on this Proposal Evaluation Form using the criteria provided below.

PROPOSAL EVALUATION CRITERIA

1. ***Research performance competence***—Capability of the investigator(s), the technical soundness of the proposed approach, and the adequacy of the institutional resources available. Please include comments on the proposer's recent research performance.

2. ***Intrinsic merit of the research***—Likelihood that the research will lead to new discoveries or fundamental advances within its field of science or engineering, or have substantial impact on progress in that field or in other scientific and engineering fields.

3. ***Utility or relevance of the research***—Likelihood that the research can contribute to the achievement of a goal that is extrinsic or in addition to that of the research field itself, and thereby serve as the basis for new or improved technology or assist in the solution of societal problems.

4. ***Effect of the research on the infrastructure of science and engineering***—Potential of the proposed research to contribute to better understanding or improvement of the quality, distribution, or effectiveness of the Nation's scientific and engineering research, education, and human resources base.

Criteria 1, 2, and 3 constitute an integral set that should be applied in a balanced way to all research proposals in accordance with the objectives and content of each proposal. Criterion 1, research performance competence, is essential to the evaluation of the quality of every research proposal; all three aspects should be addressed. The relative weight given Criteria 2 and 3 depends on the nature of the proposed research: Criterion 2, intrinsic merit, is emphasized in the evaluation of basic research proposals, while Criterion 3, utility or relevance, is emphasized in the evaluation of applied research proposals. Criterion 4, effect on the infrastructure of science and engineering, permits the evaluation of research proposals in terms of their potential for improving the scientific and engineering enterprise and its educational activities in ways other than those encompassed by the first three criteria.

SUMMARY RATINGS

Excellent: Probably will fall among top 10% of proposals in this subfield; highest priority for support. This category should be used only for truly outstanding proposals.

Very Good: Probably will fall among *top 1/3* of proposals in this subfield; should be supported.

Good: Probably will fall among *middle 1/3* of proposals in this subfield; worthy of support.

Fair: Probably will fall among *lowest 1/3* of proposals in this subfield.

Poor: Proposal has serious deficiencies; should not be supported.

CONFLICT OF INTERESTS

If you have an affiliation or financial connection with the institution or the person submitting this proposal that might be construed as creating a conflict of interests, please describe those affiliations or interests on a separate page and attach it to your review. Regardless of any such affiliations or interests, unless you believe you cannot be objective, we would like to have your review. If you do not attach a statement we shall assume that you have no conflicting affiliations or interests.

CONFIDENTIALITY OF PROPOSALS AND PEER REVIEWS

The Foundation receives proposals in confidence and is responsible for protecting the confidentiality of their contents. For this reason, please do not copy, quote, or otherwise use material from this proposal. If you believe that a colleague can make a substantial contribution to the review, please consult the NSF Program Officer before disclosing either the contents of the proposal or the applicant's name. When you have completed your review, please destroy the proposal.

It is the policy of the Foundation that reviews will not be disclosed to persons outside the Government, except that verbatim copies without the name and affiliation of the reviewer will be sent to the principal investigator. The Foundation considers reviews to be exempt from disclosure under the Freedom of Information Act (5 USC 552) but cannot guarantee that it will not be forced to release reviews under FOIA or other laws.

NATIONAL SCIENCE FOUNDATION
WASHINGTON D.C. 20550

Reference:

Dear Dr.

Our most important undertaking at the National Science Foundation is insuring that all proposals receive a fair and critical evaluation. The evaluation process includes assistance by authorities in many areas, both within advisory panels and by ad hoc review. It is our hope that you will be willing to advise us by evaluating the enclosed research proposal.

All comments you care to make regarding this proposal are welcome. Criteria to be considered in your evaluation are printed on the back of the enclosed Proposal Evaluation Form (NSF Form 1, 9/83) under "Information for Reviewers." In addition to the criteria mentioned on NSF Form 1, some proposals often require further assessment. For proposals containing experimental procedures using animals, please comment if any question is raised regarding the violation of animal welfare laws or guidelines, the exposure of animals to unnecessary pain or mistreatment, or the use of excessive numbers of animals. If the species being used is not the one most appropriate, or if alternative or adjunct methods could be used to eliminate or reduce the need for animal experimentation, please explain. For proposals that incorporate some significant aspect of applied research, please assess the potential for application of the proposed research. For proposals that are renewals of previous awards, please comment on the progress and contributions of the prior award. If you wish to make a partial evaluation of only those sections for which you have particular expertise, this will be acceptable and appreciated.

Please be sure to enter your overall rating of the merit of the proposal at the bottom of the rating sheet. Enclosed are triplicate rating sheets and a pre-addressed envelope. Please return two of these sheets, if at all possible, by the date in the upper right-hand corner or within three weeks of your receipt of this request, whichever is later. You may retain the third sheet if you wish. If a brief delay in responding is necessary, we would rather receive your review a little late than not at all. However, if you cannot review this proposal, or if your review is likely to be long delayed, please notify us promptly at the telephone number listed below or by mail. If you cannot review the proposal but know a colleague who is competent to do so, please telephone us for approval before forwarding the proposal for review.

The back of the enclosed Proposal Evaluation Form provides guidance to avoid conflicts of interests and to protect confidentiality. In addition to the guidance provided by this form, if you plan to submit a proposal of your own for review at the same panel meeting as the enclosed proposal, this constitutes a conflict of interests. In all conflict situations, please return the enclosed proposal without review, indicating your conflict of interests on the rating sheet.

Appraisal such as yours plays an important role in determining the direction of science. Thank you for assisting the National Science Foundation.

Sincerely yours,

Lewis Greenwald

Lewis Greenwald
Program Director
Regulatory Biology
202/357-7975

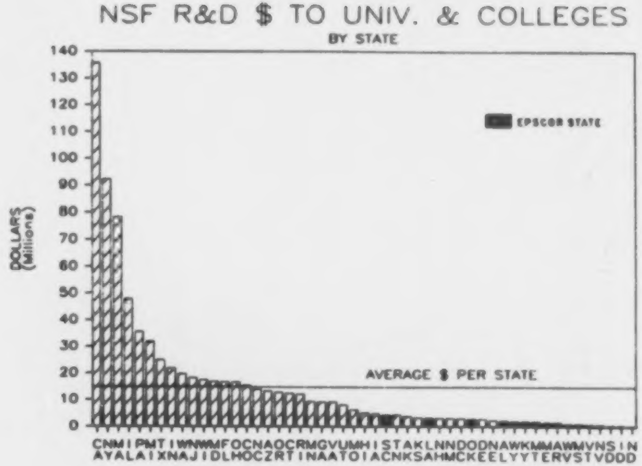
Enclosures

NATIONAL SCIENCE FOUNDATION ANALYSIS
GEOGRAPHIC DISTRIBUTION OF AGENCY FUNDING

The following charts are all based on the relative distribution of 1983 R & D funds of the National Science Foundation. The measure chosen to distribute these funds differs; the first two are commonly used to measure the relative equity of NSF's funding patterns against some standard other than science. The fourth, and last, is a presentation of the impact of NSF's standard for research excellence on its funding distribution. The third, the number of scientists and engineers, is an attempt at a halfway measure.

The substance of the analysis displayed by Charts 2, 3, and 4 is that when viewed aganist a number of different, but significant parameters, the distribution of NSF funding is clearly more equitable than Chart 1 would lead one to conclude. The data on Charts 2, 3, and 4 show that almost half of the states exceed or meet the average funding level indicated. Further, in many of those states where funding levels are significantly below the average, NSF is making a concerted effort to assist in the improvement of the scientific and engineering research and education infrastructure through such activities as the Experimental Program to Stimulate Competitive Research and enhanced support levels for undergraduate institutions in both the research and education activities of the Foundation.

CHART 1 of 4



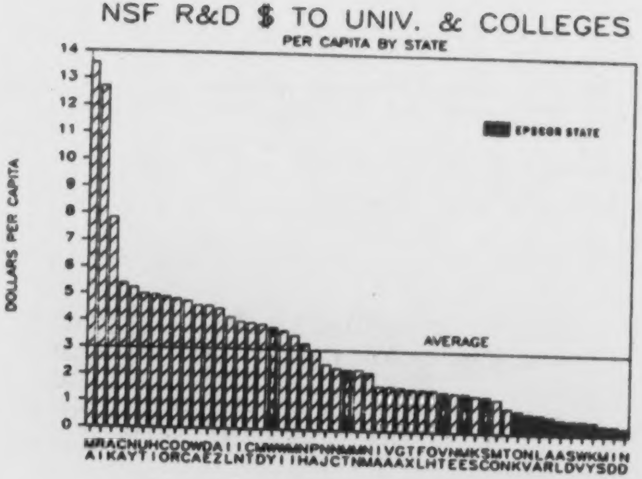
NSF R&D FUNDING TO UNIVERSITIES

AND COLLEGES BY STATE

- States are arrayed in order of receipts of 1983 NSF R&D dollars to universities and colleges.
- California, New York, Massachusetts, and Illinois are the high recipients.
- EPSCOR states represented by solid bars.

SOURCE: National Science
Foundation Feb. 1986

CHART 2 of 4



NSF R&D FUNDING TO UNIVERSITIES

AND COLLEGES PER CAPITA

BY STATE

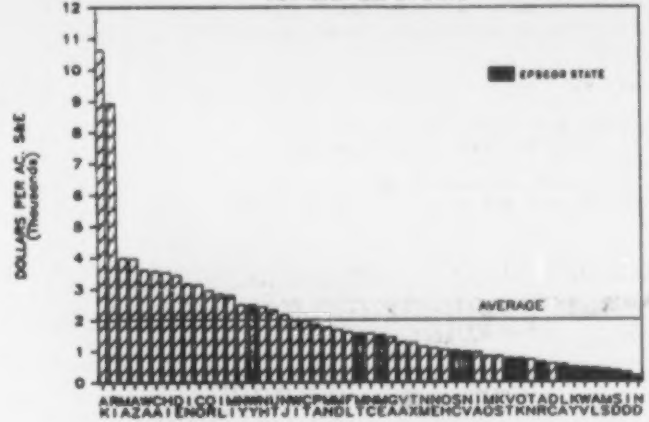
- Same NSF funding data divided by each state's population. Plotted in in descending order of per capita state funding.
- The high states are Massachusetts ($13.56/capita), Rhode Island, Alaska, California, New York, Utah, and Hawaii.
- States represented by solid bars are the states being assisted through EPSCOR.
- Twenty-two states exceed or meet the average including such states as Delaware, Arizona, Wyoming, and New Hampshire.

SOURCE: National Science
Foundation Feb. 1986

CHART 3 of 4

NSF R&D $ TO UNIV. & COLLEGES

PER ACAD. S&E BY STATE

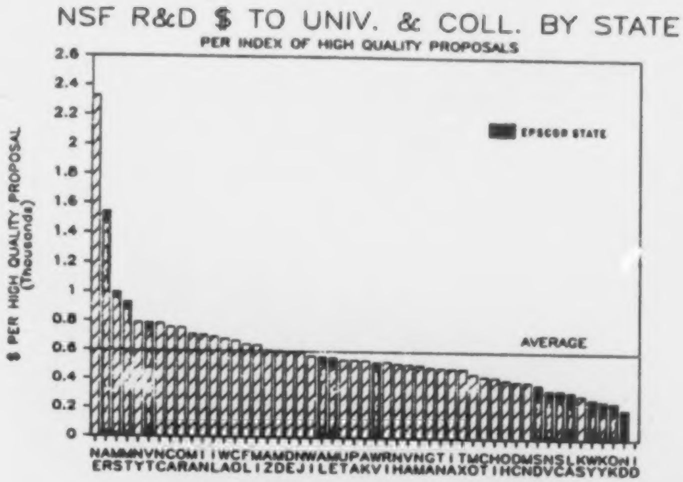


NSF R&D FUNDING TO UNIVERSITIES
AND COLLEGES PER ACADEMIC
SCIENTIST AND ENGINEER

- o NSF R&D funding divided by the number of academic scientists and engineers in each state, arranged in descending order.
- o The high states are Alaska, Rhode Island, Massachusetts, and Arizona.
- o States represented by solid bars are the states now being assisted through EPSCOR.
- o Twenty states exceed or meet the average including such states as Colorado, Minnesota, and Utah.

SOURCE: National Science
Foundation Feb. 1986

CHART 4 of 4



NSF R&D FUNDING TO UNIVERSITIES AND COLLEGES DIVIDED BY NUMBER OF HIGH RATED PROPOSALS PER STATE

(PROPOSAL QUALITY INDEX)

- NSF R&D funding per state divided by a sample of high rated proposals from each state in the years 1980, 1981, and 1982.
- The chart shows that, in general, NSF's allocation of R&D obligations to academic institutions is proportional to the highly rated proposals coming from a state.
- States represented by solid bars are the states being assisted through EPSCOR.
- Nineteen states exceed or meet the average including such states as Nebraska, Arkansas, Mississippi, Montana, Florida, and North Carolina.

SOURCE: National Science
Foundation Feb 1986

APPENDIX 3—ADDITIONAL MATERIALS SUBMITTED FOR THE RECORD BY DR. NEVILLE P. CLARKE

Research
1984

The State Agricultural Experiment Stations

The Experiment Station Committee on Organization and Policy
The Cooperative State Research Service

Agricultural research is an integral factor contributing to the success of U.S. agriculture. This booklet outlines the role of the State Agricultural Experiment Stations and the Cooperative State Research Service, in the total U.S. agricultural research effort.

Scope

- **The State Agricultural Experiment Stations: Structure, Performance, Funding**
- **Agricultural Research Today: Opportunity & Challenge**
- **The Planning Process: Issues, Priorities, Solutions**
- **Agricultural Research Priorities: An Action Agenda**

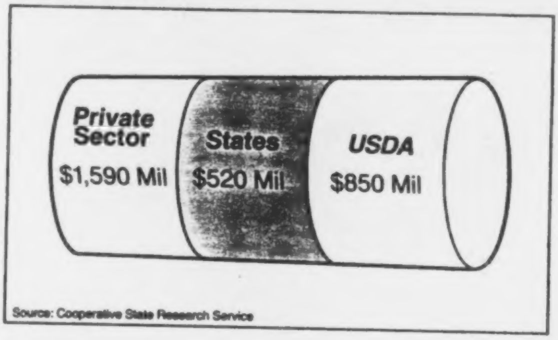
The State Agricultural Experiment Stations: Structure, Performance and Funding

Agricultural Research in the United States

Publicly supported agricultural research is conducted in a relatively centralized system, principally in the USDA, which is coupled to a decentralized system of research in the states. The state agricultural experiment stations' (SAESs) research programs are funded with both federal and state funds. Agricultural research and development is substantial; increasingly funds from the private sector are being invested in certain aspects of biotechnology research in universities.

U.S. Agricultural Research

Together, the three parts of the total research system invested about $3.0 billion in research in 1982



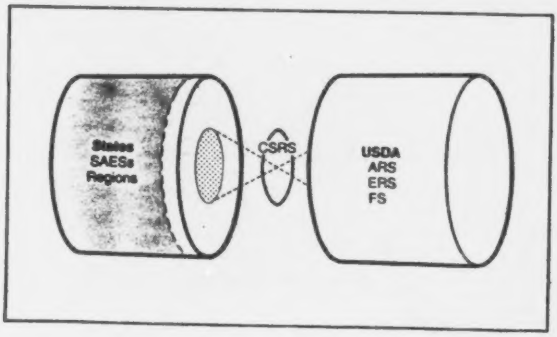
Source: Cooperative State Research Service

The State-Federal Partnership

The Hatch Act, passed in 1887, followed by the Evans-Allen and McIntire-Stennis legislation, established a unique state-federal relationship in research that has provided the new knowledge necessary to make U.S. agriculture the most productive in the world. Through joint federal-state funding, the state agricultural experiment stations are linked to federal research agencies, the Agricultural Research Service (ARS), the Economic Research Service (ERS), and the Forest Service (FS), in collaborative and complementary programs of research to meet local, regional, and national goals.

The Partnership

Close coupling exists between state research programs and federal agencies; the Cooperative State Research Service (CSRS) provides focus



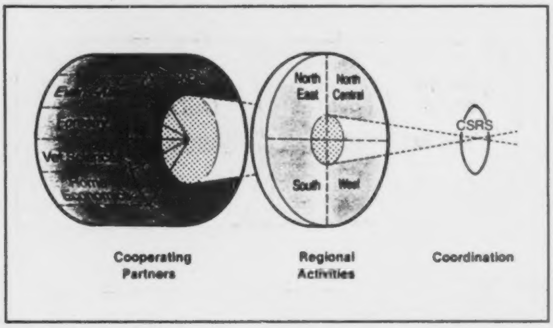
A principal focal point for the state-federal partnership is the Cooperative State Research Service (CSRS), the federal agency that administers USDA funds appropriated by Congress for the states. CSRS acts as a lens focusing the broad programs of agricultural research in the states and facilitating communication and planning with other federal agencies.

The Cooperating Partners

The cooperating partners participate in programs of agricultural research in the state agricultural experiment stations, the Evans-Allen Institutions (historically, the black land grant universities), as well as in the programs of research in forestry, home economics and recently, animal health. The institutional relationships of these partners in their home states are variable. Generally the state agricultural experiment stations, being the largest and most diversified of the partners, tend to act as a central clearinghouse for the elements within the partnership.

Partner Relationships

The regional framework provides essential management infrastructure

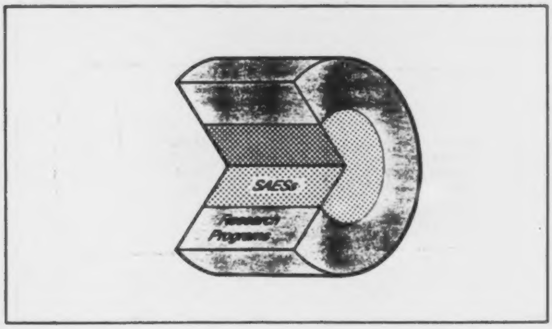


The Regional Structure

The four regional associations of agricultural experiment station directors provide an infrastructure for managing that portion of the federal formula funds earmarked for regional research. They also engage in planning and organizing broad programs of regional and interregional coordination and collaboration. These associations enhance the effectiveness of the system by sharing resources on problems common within and among regions. In addition, the regional associations initiate the process of consolidating state research needs and priorities for aggregation at the national level. The Cooperative State Research Service also acts as a national focal point for these regional activities.

State Research Programs

The Agricultural Experiment Stations are the major research partners in the states



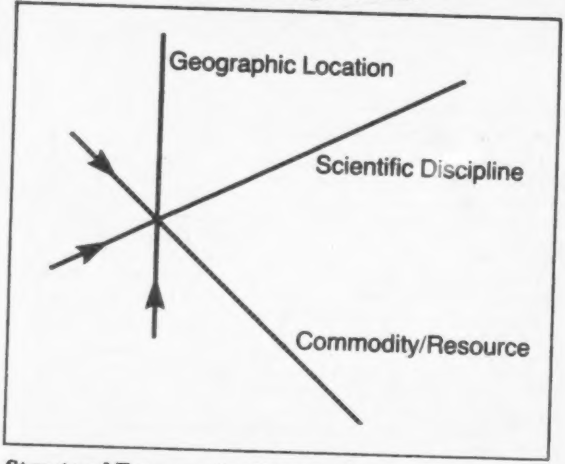
The State Agricultural Experiment Stations

The Hatch Act provided the opportunity for a state-federal partnership and, in all cases, an individual agricultural experiment station emerged in every state. As part of the land grant system these stations are closely involved with counterpart programs of cooperative extension and with the academic programs in agriculture, veterinary medicine, forestry and home economics. The experiment stations conduct targeted research which produces the knowledge and technology used to both avoid and solve problems of agricultural producers, processors, and consumers of food.

The key conceptual element in the partnership strategy is recognition that the enormous diversity in U.S. climate and geography results in an equally diverse system of agricultural production. The state experiment stations are committed to serving the research needs of this diversified agriculture within the individual states. Not all research needs are totally site specific; some are pervasive. The total system, supported with joint funding, works as a coalition of federal, regional and state activities to meet national needs as well as state and regional needs.

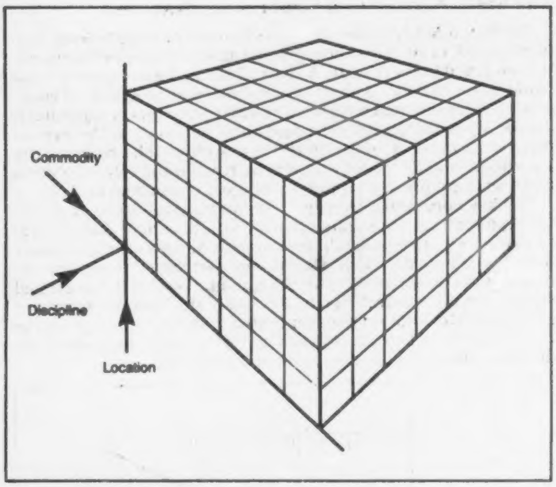
Research Framework

SAES programs of research have three basic structural components



Structural Framework of Research

A simplified picture of the structure of research in the experiment stations would have three coordinates: (1) geographic location, (2) commodity and resource, and (3) scientific discipline. The current structure of research in the state agricultural experiment stations evolved as a result of several interrelated needs. First, the diversity of climatic and geographic conditions has required a degree of site-specificity in agricultural research that, to some, appears as duplication of effort. A number of states, where diversity is extreme, conduct research related to a very large number of economically significant agricultural commodities and resources. Solutions to contemporary problems require research involving an increasing number of disciplinary specialties applied both separately and within multidisciplinary teams.



The Base Program of Research

In the aggregate, the broad and diverse programs of research in the experiment stations along with related efforts in academic programs and extension fill out the structural framework of the system. This combination comprises the essential agricultural experiment station *base program* of research. The base program is crucial and must be maintained, updated, invigorated and nurtured for the good of agriculture in the state, region and country. The base is not static, it is a constantly changing set of individual research projects. The dynamic nature of the base program is difficult to fully appreciate because of low visibility of the smaller programs at the national level. The state agricultural experiment stations continue to place highest priority on maintaining the health and vigor of the base program of research. With a dynamic base program comes opportunity to exploit new initiatives, to meet the challenge of new problems, and to take advantage of new technologies.

The Base Program

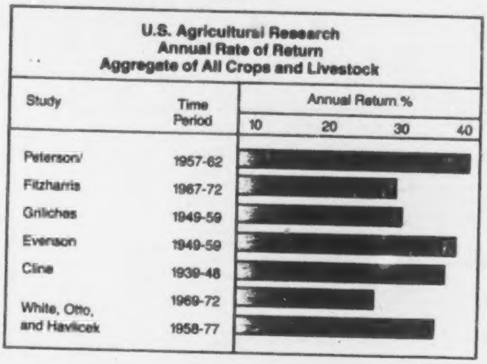
Broad programs in research, linked to interrelated academic and extension activities, comprise the base program

The Payoff of Agricultural Research–Highlights

Return on Investment

Economic returns to society from agricultural research are very favorable

- *Average rates of return to public sector agricultural research investments have been 30 to 50 percent annually since the late 1930's.*



- *Public sector research has accounted for about one-fourth of the total productivity growth in U.S. agriculture over the last four decades.*

- *For every one dollar of benefit acquired through research in a state or region, about $4 in benefits spill over into nearby states and regions.*

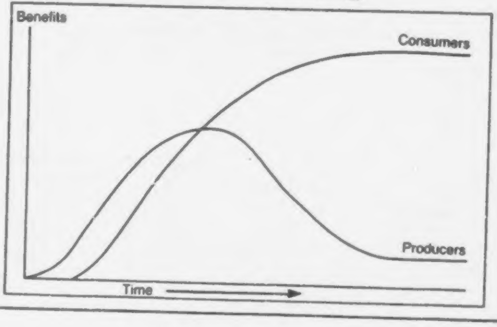
- *Public sector investment in production-oriented agricultural research is about 1.0 percent of total value added in farming.*

- *Public sector agricultural research tends to benefit lower income groups preferentially.*

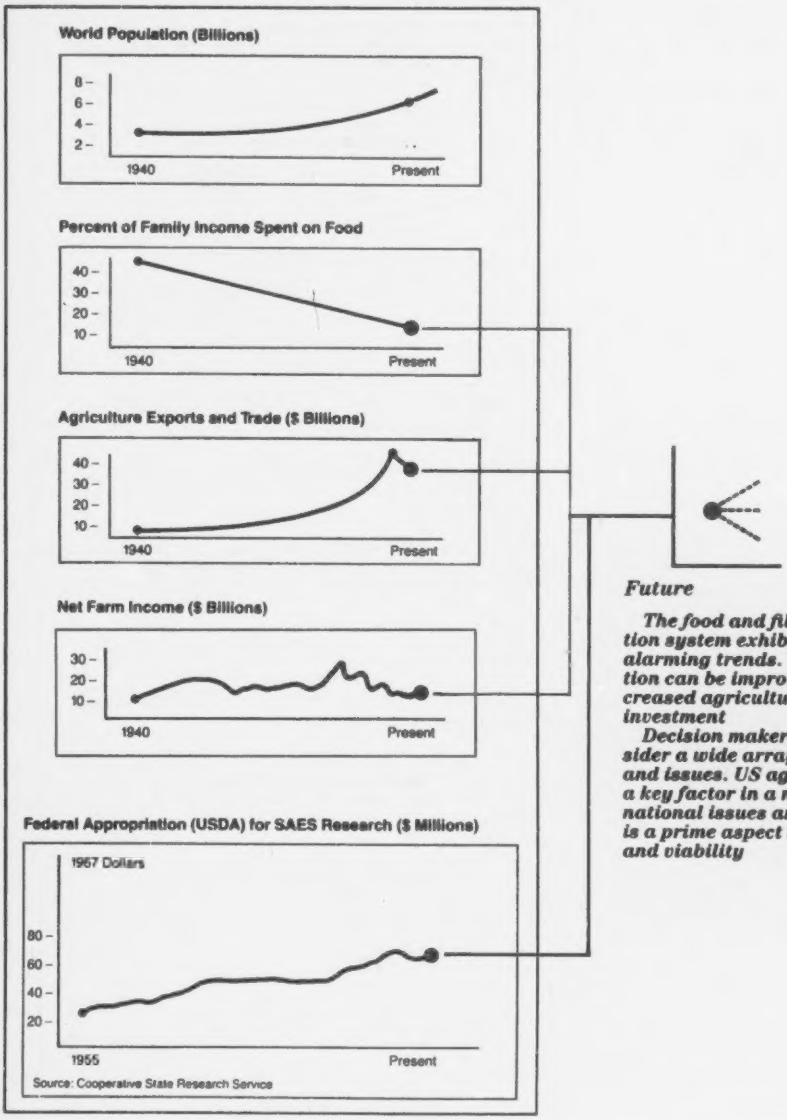
Consumer Benefit

Research related reductions in cost of production are transferred to consumers

Benefits from New Technology



Pervasive Issues–Hard Choices



Source: Cooperative State Research Service

Future

The food and fiber production system exhibits several alarming trends. This situation can be improved with increased agricultural research investment

Decision makers must consider a wide array of needs and issues. US agriculture is a key factor in a number of national issues and research is a prime aspect in its health and viability

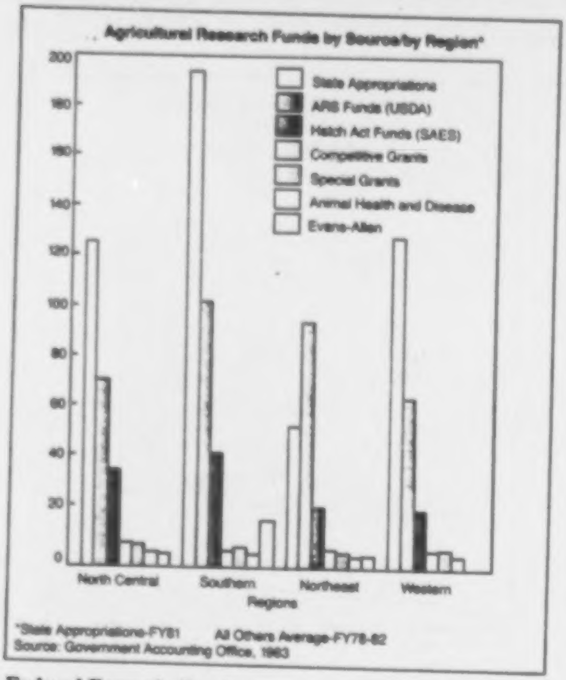
The Funding Picture

The U.S. agricultural research system today is supported by a mixture of federal, state, and private monies. Several significant trends have changed the traditional funding pattern in recent years and this is reflected in contemporary budgets of the agricultural experiment stations.

Hatch Funds

They are a smaller, but important share of the total research budget today

Agricultural Research Funds by Source/by Region*



*State Appropriations-FY81 All Others Average-FY78-82
Source: Government Accounting Office, 1983

Federal Formula Funds

Federal formula monies allocated on the basis of number of farms and rural population within a state (Hatch Act), originally provided the major source of support for state agricultural research. Today, these funds account for a smaller, but important, share of the total research budget in major agricultural states.

Hatch Funds

These funds provide stability and flexibility

State Agricultural Experiment Stations
Budgeted Funds
($ × 1000)

Source	FY78	FY79	FY80	FY81	FY82	FY83	FY84
Hatch*	$109,098	$109,088	$118,969	$128,615	$141,109	$149,295	$152,291
Federal**	143,148	174,396	188,031	200,887	221,218	244,949	247,656
State	367,847	406,147	446,945	490,967	522,213	—	—

*Includes Overhead **Excludes CGCA and Other Federal
Source: Cooperative State Research Service

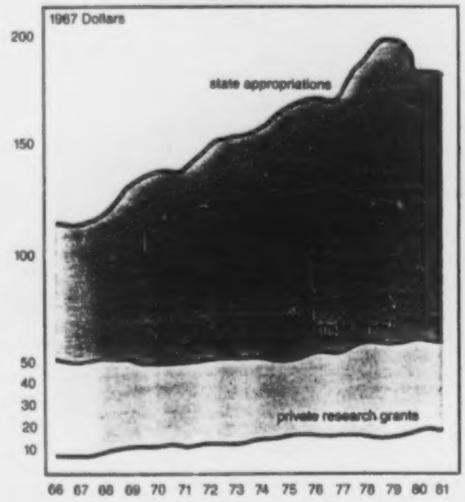
Hatch funds provide necessary long-term, stable research support and offer research managers the flexibility to adjust and redirect efforts toward critical issues. These funds offer the greatest opportunity to employ the important linkage in the state-federal research partnership.

A portion of the federal formula funds is targeted for regional research projects supporting joint efforts by states sharing geographic proximity and needs. The total funding involved in regional research in modern times is much larger than the twenty-five percent of formula funds mandated by the Hatch Act.

The Present Picture

The states are a principal source of public support for agricultural research today

State Agricultural Experiment Stations
Funds by Source
$-Millions



Source: Cooperative State Research Service

States funds, originally required to simply 'match' federal formula funds, are now the most important source of public funds for agricultural research and total over four times the federal contribution.

Competitive Grants

Competitive grant monies supplement the larger research effort and are becoming essential for sustaining selected basic research programs. Because of limited resources, the present competitive grants program is narrowly focused on a few key areas. Because agencies other than the USDA offer grant monies in areas appropriate for basic agricultural research, SAES scientists are competing successfully for support from these sources. A substantial and broad-based agricultural science competitive grants program is needed to capture new opportunities and implement major multidisciplinary initiatives.

Special Grants

Special grant monies are intended for application on problems, perceived by Congress, as requiring immediate attention. The resources for such targeted research are directed to the State Agricultural Experiment Stations. The special grant funds have allowed focus on both broad and site-specific issues over the years and offer a special capability to the Congress for handling specific identified issues.

Private Sector Monies

Private industry participates in agricultural research using two distinct methods; industry may conduct separate and proprietary programs of research or may fund or collaborate in the research efforts of the state agricultural experiment stations. Because of the capabilities found in the public agricultural research institutions and the rising cost of conducting modern research, private industry is increasingly supporting experiment station programs. With increasing frequency many unique arrangements and agreements are being established between industry organizations and the stations with varying degrees and types of benefits for both parties. The enlarging scope of this industry-station relationship offers substantial benefits to agricultural producers and consumers. Effective industrial-SAES relationships can enhance the productivity of both parties and contribute to more effective technology transfer to producers through industrial development and marketing.

Agricultural Research Today:
Opportunity and Challenge

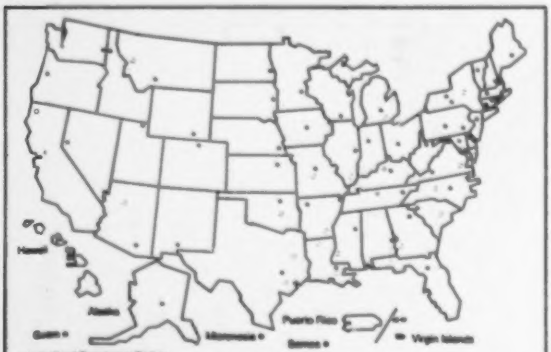
The basic institutional arrangement and organization of agricultural research in the United States have remained essentially the same for over a century. A contrasting aspect of this outwardly apparent sameness is the system's proven internal capacity to recognize opportunity and to respond to challenge. By continuing to mobilize and redirect resources, and to develop new research agendas, the inherent strength of the system's structure has been exploited to produce new knowledge, services, and technologies.

Aggressive marketing can sell an old product in a new wrapper; U.S. agricultural research has, without fanfare, continually improved its product while retaining traditional packaging. Significantly, emerging large-scale societal trends are following the path toward decentralization and local initiative, the historical essence of the U.S. agricultural research system.

Decentralization–Strength of the System

> ***The people of this country are rebuilding America from the bottom up... decentralization... has transformed politics, business, our very nature.***
>
> ***John Naisbitt***
> ***Megatrends***

The decentralized agricultural research system in place today operates distributed research programs in each state. The state agricultural experiment stations operate at the grassroots level and possess unique communication linkages with local clientele groups.



● Agricultural Experiment Station
○ 1890 or Cooperating Forestry and Veterinary College
Source: Cooperative State Research Service

At least one Agriculture Experiment Station exists in every state in the Union and in Puerto Rico, the Virgin Islands, American Samoa, Guam, Micronesia, and the District of Columbia. In addition to the main station in each state, there often exist several centers or field stations for conducting research in distinct production regions.

SAESs and Partners

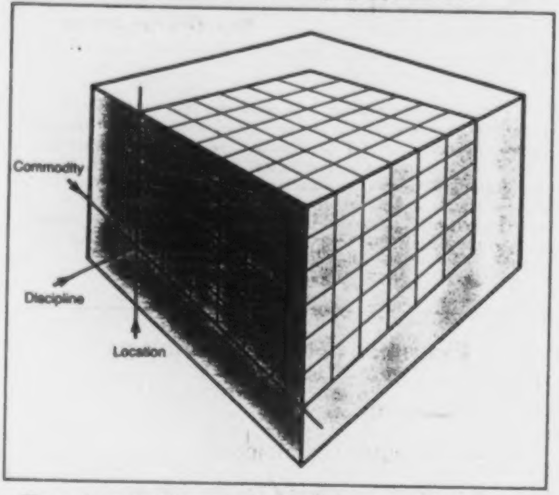
This distributed system of research has contributed much to the growth of American agriculture

Site-specificity is essential if new knowledge and technologies are to be successfully adapted to the thousands of differing production niches that characterize agriculture. By nurturing this distributed system, appropriate scientific response can be made to local problems. The systems' diversity offers considerable opportunity to recognize and capture emerging opportunities.

Opportunities and the Base Program

Opportunities exist in all dimensions of the base program

Benefits will continue only if the base program is strong and flexible



Through contact between client and station scientist, new issues and problems enter the national research system. These issues and problems are evaluated in the light of available science and technology, and new opportunities begin to emerge. Priorities and resources become the primary consideration when assessing and choosing opportunities. Selection of opportunities is dependent upon prioritizing needs, considering available resources, and evaluating the existent or new technologies which can be applied to a research effort.

Technology in Contemporary America

> ***We have emphasized applying existing knowledge and failed to replenish our intellectual capital.***
>
> ***Rep. George Brown***

The future growth of the U.S. economy depends primarily on those sectors able to sustain technological leadership in the world. A major component of America's agricultural success story has been the development and implementation of new technologies and resources to boost production. Because of resource limitations at home and fierce competition abroad, agricultural research must now make full use of a new generation of scientific and technical resources to develop production technologies which result in more efficient use of land, water, and energy.

Examples of New Technology

- Biotechnology
- Computer Technology
- Communications Technologies
- Remote Sensing/Satellite Imagery
- Biocontrol
- Robotics
- Alternative Energy Sources

Substantial opportunity exists in agricultural research to develop both new knowledge and new technology as well as to exploit advances made in other sectors. Articulation between new knowledge and technology development is a vital aspect in capturing these opportunities. The state agricultural experiment stations offer a favorable institutional system to develop and link fundamental knowledge to ongoing applied research programs, to technology development, and to knowledge transfer avenues. Clientele input and feedback as well as continuing scientist evaluation, assure program balance and aid prioritization of opportunities.

Many of today's emerging technologies will have immediate and increasing impact on agricultural research and on the production, processing, and marketing of food and fiber. Some of these technologies will find their most important role in agriculture as supplements to traditional research. Two examples of such technology are biotechnology and computers.

Biotechnology and Electronic Technology–An agricultural perspective on two rapidly emerging technologies

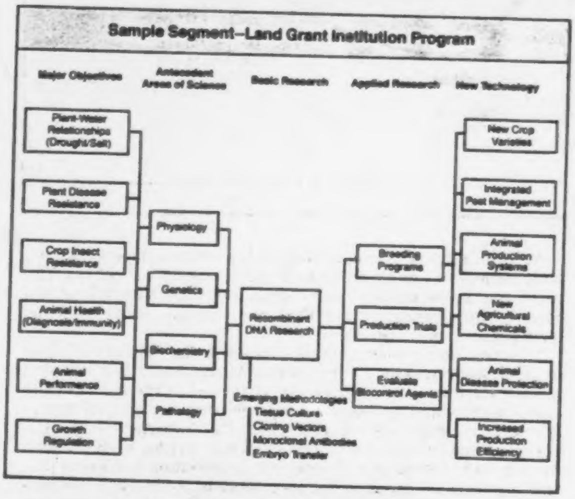
---

> ***Biotechnology is a central overlay of...crop and animal priorities...because of the potential to significantly lessen our dependency on expensive capital, energy, and natural resources.***
>
> ***Users Advisory Board***

Biotechnology–Production efficiency in the decades ahead will come to depend more on biological innovation than upon additional resource applications. Through biotechnology, agricultural scientists will expand their abilities to manipulate mechanisms which govern the metabolism, growth, and development of plants, animals, and micro-organisms. Benefits from such advances will find rapid application along broad fronts of agricultural research and production.

The Land Grant Advantage

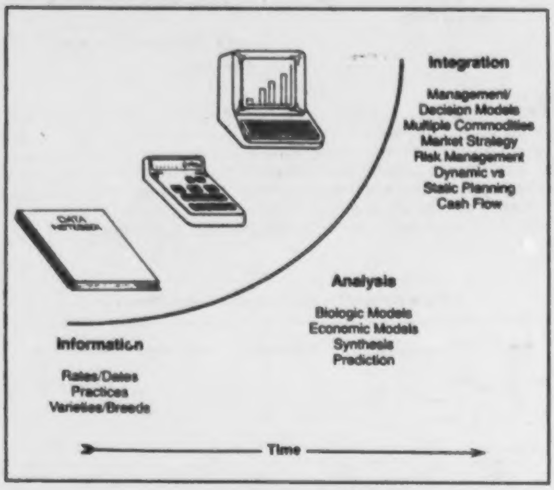
State agricultural experiment stations have an existing infrastructure to guide and exploit research in biotechnology


Sample Segment–Land Grant Institution Program
Major Objectives
Antecedent Areas of Science
Basic Research
Applied Research
New Technology
Plant-Water Relationships (Drought/Salt)
Plant Disease Resistance
Crop Insect Resistance
Animal Health (Diagnosis/Immunity)
Animal Performance
Growth Regulation
Physiology
Genetics
Biochemistry
Pathology
Recombinant DNA Research
Emerging Methodologies
Tissue Culture
Cloning Vectors
Monoclonal Antibodies
Embryo Transfer
Breeding Programs
Production Trials
Evaluate Biocontrol Agents
New Crop Varieties
Integrated Pest Management
Animal Production Systems
New Agricultural Chemicals
Animal Disease Protection
Increased Production Efficiency

Success in the application of new biotechnology research technologies requires integration of the new discoveries in basic sciences into the disciplines of traditional agricultural research. The existing agricultural research system in the land grant university setting provides an advantageous situation for applying advances in biotechnology to agriculture. This system supplies scientific expertise in the requisite antecedent areas of science as well as interrelated programs of applied research, site specificity, and technology transfer capabilities.

Electronic Technology–New computer technologies are rapidly changing society, agriculture and agricultural research included. Opportunities based on these technologies exist for the scientist conducting research, for the producer in business management and, for all parties–scientist, producer, and consumer–in information and technology transfer.

Significant need and opportunity exists for agricultural scientists to develop software for production management systems in all phases of agriculture.


Integration
Management/
Decision Models
Multiple Commodities
Market Strategy
Risk Management
Dynamic vs
Static Planning
Cash Flow
Analysis
Biologic Models
Economic Models
Synthesis
Prediction
Information
Rates/Dates
Practices
Varieties/Breeds
Time

Evolution of Decision Aids

The SAESs have a fundamental role to play in acquiring biological and economic information and developing decision models

Agricultural production is inherently complex, depending on a wide variety of inputs, some more predictable than others. The revolution in technology offers agriculture new alternatives and opportunities. Strategies to reduce and manage risk are vital elements of successful production.

The computer provides a tool which is being rapidly exploited to meet modern management needs. Management and decision models have evolved over recent years and in the next decade computer-assisted decision making will become a part of the operation of most successful enterprises. The State Agricultural Experiment Stations have a fundamental role to play in developing the relevant biological and economic information needed to provide appropriate input to decision models, to develop and produce relevant software for specific local and regional needs and to assist in assuring that an effective transfer process occurs so that users can have ready access to the new management tools.

The U.S. agricultural research system is an information producing and disseminating system; computers and other electronic technologies are dramatically changing the entire communications industry. Substantial opportunity exists for the state agricultural experiment stations to improve communications between researchers, and with clientele, extension personnel, and the media, using new communications technologies.

Competitive Grant Program

> ***My own preference would be to try to build up programs that let all scientists compete for funds for agricultural research and training of students...assuring such growth will not occur at the expense of ongoing programs.***
>
> ***George Keyworth***

Despite the relatively low profile of the agricultural sciences, many agricultural scientists are working on the cutting edge of their disciplines. Because of the quality of science, institutional linkages, strong multi-disciplinary posture, and technology transfer capabilities, the state agricultural experiment stations encourage efforts to expand federal competitive grants programs in the agricultural sciences. New grant monies, however, must not be secured at the expense of the SAES base program. A strong and expanding competitive grants program offers opportunity to use the inherent strength of the system on focused areas of national priority.

Biotechnology in the SAESs

The SAESs are presently involved in nearly 600 biotechnology projects funded at a level of approximately $50 million

Biotechnology in the State Agricultural Experiment Stations
1983

Projects	Personnel		
	Scientists	Students	Staff
579	263	442	498

Projected Additional Short-term Commitments (July 1984)

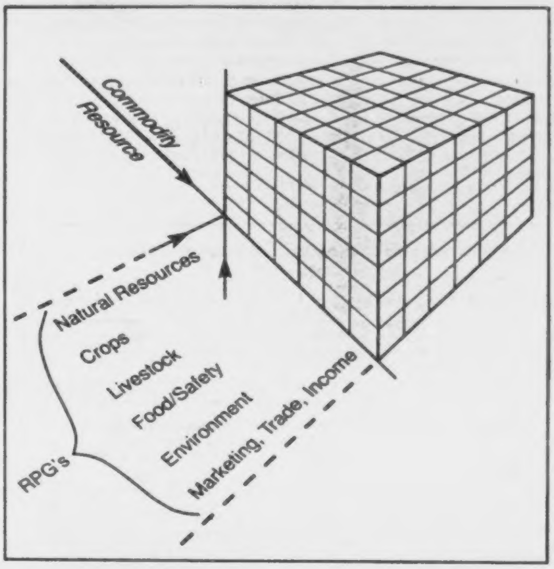
Scientists	Students	Staff
108	161	118

Using biotechnology as an example, the agricultural experiment stations are prepared to play a major role in the emerging technology arena. Today, when scientific investigation requires broad, multi-disciplinary teams, the experiment stations have substantial manpower capabilities. The projected additional SAES commitments to new personnel in biotechnology is indicative of perceived potential and underscores the dynamics of the system. The land grant institutions, encourage a competitive grant biotechnology program in the USDA.

The related educational benefit of conducting fundamental research in the experiment stations is a significant spinoff. Coupled with the broad problem solving orientation inherent in the mission, the stations offer tremendous educational opportunity for intensive, practical, hands-on education for both undergraduate and graduate students. In addition, the broad scientific approach to problem solving has required assembling multidisciplinary research teams within experiment stations. Today, when all facets of scientific investigation demand such an approach, the agricultural research system is postured to meet this need.

The Planning Process
Issues, Priorities, Solutions

The U.S. agricultural research system consists of interdependent institutions and groups, functioning on the state, regional, and federal levels. The state agricultural experiment stations operate at the grassroots level of this system and provide an entry point for emerging issues. The priority given various issues within the system is affected by differing concerns and perspectives in each separate organization and on each level.



A Cataloging Scheme

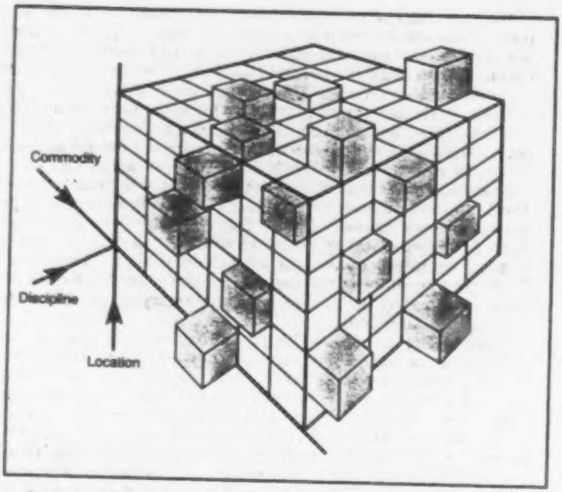
Research Planning Groups (RPGs) provide a common language for communicating between state, regional, and federal levels

To facilitate communications, a common language, essentially a cataloging scheme, has been devised with sufficient breadth to apply at the federal level, yet retain a sense of the specificity of local needs. In this scheme, issues and needs are clustered into Research Planning Groups, (RPGs). Using these groupings it is possible to begin the process of generalization at the experiment station level. It is the sum total of this state input with further sequential generalization and increasing political flavor which emerges as a national agricultural research plan. Much of this process is internal to the various distributed partners so the vital planning activity has little external visibility.

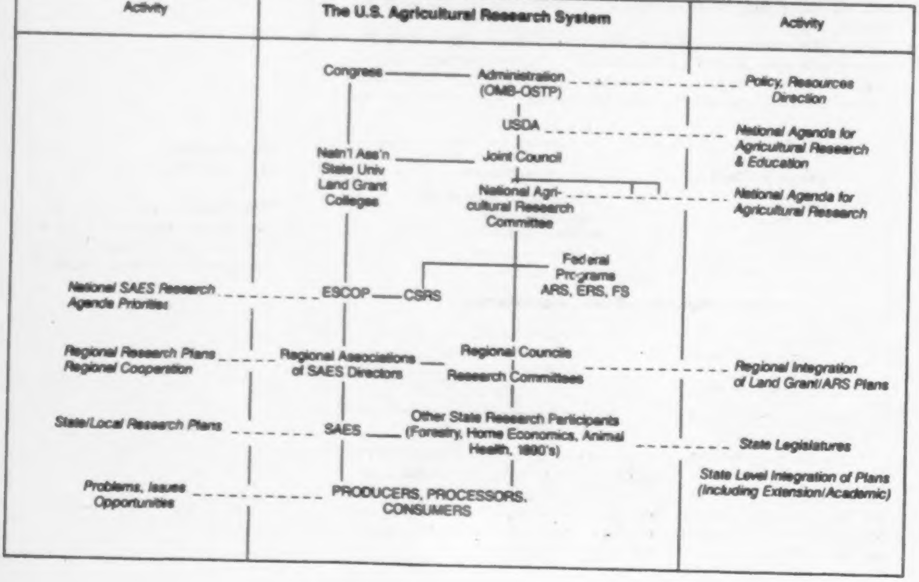
Opportunities for progress and improvement exist for every commodity or resource, within each discipline, and at all locations. Though the individual needs of any single state program become less visible with aggregation and prioritization of broad issues, common or general needs begin to surface. Consideration of broader needs, impact, and costs begin to allow ranking of opportunities beside pressing national needs.

Priority Opportunities

Opportunities develop from the base program and are prioritized on the basis of need, available resources, and probable return


Commodity
Discipline
Location

Opportunities emerge from the frontier research programs in the base program. From the multitude of recognized opportunities and issues, priority ranking of goals and needs develops. Ultimately, selected opportunities are melded with needs of the total agricultural research and education system. The success of the system depends on a dynamic base program which has the flexibility and the resources to respond to opportunities as they emerge.


Activity
The U.S. Agricultural Research System
Activity
Congress
Administration (OMB-OSTP)
Policy, Resources Direction
USDA
National Agenda for Agricultural Research & Education
Natn'l Ass'n State Univ Land Grant Colleges
Joint Council
National Agricultural Research Committee
National Agenda for Agricultural Research
Federal Programs ARS, ERS, FS
National SAES Research Agenda Priorities
ESCOP
CSRS
Regional Research Plans Regional Cooperation
Regional Associations of SAES Directors
Regional Councils
Research Committees
Regional Integration of Land Grant/ARS Plans
State/Local Research Plans
SAES
Other State Research Participants (Forestry, Home Economics, Animal Health, 1890's)
State Legislatures
State Level Integration of Plans (Including Extension/Academic)
Problems, Issues Opportunities
PRODUCERS, PROCESSORS, CONSUMERS

The State Agricultural Experiment Stations are the major entry point within the research system for emerging issues. State plans are aggregated at the regional level and by the Experiment Station Committee on Organization and Policy (ESCOP) and CSRS at the national level. At the state and regional levels plans of other participants in the land grant system are integrated and taken to the next level. The National Agricultural Research Committee (NARC–part of the Joint Council structure) integrates the federal and state inputs for agricultural research. The Joint Council integrates the total agricultural research and education plan at the national level. The Joint Council and the Secretary of Agriculture receive input from the User's Advisory Board. This overall system, a combination of (1) long standing functions of ESCOP and CSRS and (2) more recent interactions of the Joint Council, has increased its effectiveness recently, particularly as a result of the leadership and focus provided by the new Assistant Secretary of Agriculture for Science and Education.

The ongoing decision-making pattern shapes the general form and magnitude of research response at regional and state levels. Collaborative regional or state action may be appropriate or individual state programs tailored to specific state and local settings may emerge. Regional research is coordinated through the Committee of Nine with interactions at the Cooperative State Research Service to insure continuity, quality and effective reporting.

The SAESs alter or fine tune programs to accommodate many emerging problems. Issues broad in scope proceed through the system until an appropriate combination of scientists, partners, missions, and resources can be established. Increasing responsibilities and diminishing federal support have forced stringent priority-setting efforts at all levels. At the state level prioritization is often complicated by unique natural- or economics-related circumstances while political considerations play an increasingly prominent role at higher levels. Increased demands for evaluation and accountability have accompanied shrinking federal support monies.

Bench scientists also play an important role in establishing research priorities. Their determinations of significance, estimate of required resources, and assessment of potential for solution, are essential during priority-setting efforts on all levels of the system.

Expanding Responsibilities

The traditional clientele has been augmented by new groups requesting increased attention for their needs

Historical Clientele

- Farmers
- Ranchers

New Clients

- Urban/Suburban Dwellers
- Part-time Farmers
- Environmentalists
- Consumer Groups

Growing Complexity of SAES Responsibilities

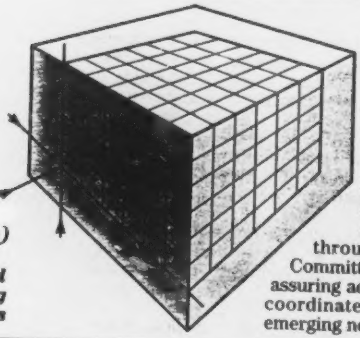
- Marketing
- Suppliers and Services
- Import/Export
- National Farm Policy
- Consumer Issues
- Environmental Protection
- Conservation

The bottom-up priority-setting process, a historic strength of the U.S. agricultural system, has been complicated in recent years by incorporating the concerns of new clientele groups with differing interests and values. This situation, coupled to diminishing federal support monies, has placed extreme pressures on the priority-setting actions at every level of the system.

Agricultural Research Priorities:
An Action Agenda

The state agricultural experiment stations research agenda which emerges from the priorities perceived at state, regional, and federal levels addresses two major issues, they are:

- ***strengthening the dynamic capabilities of the research system's base program,***
- ***identifying and capturing priority opportunities.***



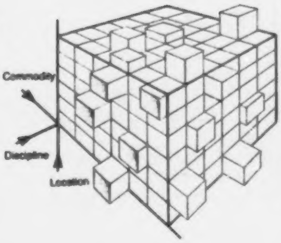
The Base Program

The research priorities which can be addressed within the SAESs base program are catalogued in Research Planning Groups (RPGs). Concerns and needs enter the research system on a continuous basis through periodic review at state and regional levels, as well as through the Experiment Station Committee on Organization and Policy, assuring adjustment within the RPGs and coordinated allocation of resources to emerging needs.

Research Programs (RPs)

The RPs are evaluated and realigned with emerging issues every two years

RP	Title
1.01	Soil and Land
1.02	Water & Watersheds
1.03	Recreation
1.04	Environmental Quality
1.05	Weather Modification
1.06	Fish & Wildlife
1.07	Remote Sensing
1.08	Aquatic Weeds
1.09	Natural Resources
2.01	Inventory and Appraisal of Forest Resources
2.02	Timber Management
2.03	Forest Protection
2.04	Harvest Processing and Marketing of Forest Products
2.05	Forest Watersheds, Soils, Pollution
2.06-2.09	Forest Amenities and Alternate Uses
2.10	Trees, Forest and Forest Production
2.11	Forest and Range Resources
3.01	Corn
3.02	Grain Sorghum
3.03	Wheat
3.04	Other Small Grains
3.05	Rice
3.06	Soybeans
3.07	Peanuts
3.08	Sugar Crops
3.09	Forage, Range and Pasture
3.10	Cotton & Cottonseed
3.11	Tobacco
3.12	New Crops and Minor Oilseeds
3.13	Fruits & Nuts
3.14	Vegetables Crops
3.15	Natural Beauty
3.16	Bees
3.17	Weeds in Crops
3.18	Seeds
3.19	Non-Specific Plants
4.01	Beef Cattle
4.02	Dairying
4.03	Poultry
4.04	Sheep
4.05	Swine
4.06	Horses & Other Animals
4.07	Aquatic Food Animals
4.08	Non-Specific Animals
4.09	Invertebrates
5.01	Individuals and Families
5.02	Living Environment
5.03	Communities Institutions and Services
5.04	Insects Affecting Man
5.05	Research on Administrators of Research
6.01	Farm Adjustment, Prices and Income
6.02	Foreign Agriculture Trade and Economic Development
6.03	Marketing and Competition
7.01	Man-made Resources
7.02	Technology not associated with Special Commodities, Resources or RPG's
8.01	Human Nutrition
8.02	Food Processing
8.03	Food Safety
8.04	Food Storage, Distribution and Marketing
8.05	Food Service



Priority Opportunities

Emerging from a myriad of recognized opportunities are several significant priority opportunities offering substantial return on investment and addressing critical national issues.

Biotechnology–Research is moving the agricultural production system towards a new science based on the capacity to manipulate and modify plants and animals, using recombinant DNA and other new technology.

Electronic Technology–Rapidly developing electronic technologies are adding capacity and new capabilities to modern research. The incorporation of computer technology in agricultural production also offers significant new management and communications tools.

Natural Resources–New technologies, incentives and production systems must be devised to increase efficiency of resource use and encourage conservation of natural resources.

Food–Safe and wholesome foodstuffs are major national concerns. Improved production practices, new storage and preservation techniques, and thorough understanding of nutrition are pressing research needs.

Environment–Substantial need exists for full ecological understanding of agricultural production system components and increased awareness of social implications in technology development.

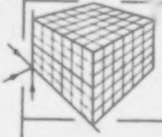
Agricultural Policy and Foreign Trade–Maintenance of strategic agricultural strength requires a federal policy which links the economic strength of agriculture, production efficiency, with the best potential markets, export sales.

The National Agricultural Research Agenda

The SAES Agenda

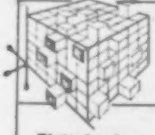
The SAES and ARS plans are combined with input from the other research partners

The SAESs



The Base Program (RPGs)

Crops
Livestock
Natural Resources
Environment
Food/Safety
Marketing, Trade, Income



Priority Opportunities

- **Biotechnology**
- **Computer Technology**
  - **Risk Management**
  - **Marketing**
  - **Communications**
- **Natural Resources**
  - **Production Systems**
  - **Conservation**
  - **Use Efficiency**
- **Food**
  - **Safety**
  - **Nutrition**
  - **Storage**
- **Environment**
  - **Special Issues**
  - **Agricultural Residue**
  - **Biocontrol**
- **Agriculture Policy/Foreign Trade**

+

ARS

Program Plan

Plant Productivity and Quality
Animal Productivity and Quality
Soil and Water Conservation
Integration of Systems
Adequate Human Nutrition
Commodity Conversion and Delivery

Six-Year Plan Emphasis

Integrative Systems
Commodity Conversion and Delivery
Human Nutrition
Soil and Water Conservation
Efficiency of Production

Other Federal Research Partners

Forest Service

Economic Research Service

+

Other State Research Partners

=

Joint Council

Research Agenda

Basic Biotechnology

Communications Technology

Sustaining Soil Productivity

Water Management

Forest, Range, Pasture Productivity Enhancement

Analysis of Price and Income, Policy, and Foreign Trade

Human Nutrition

Scientific Expertise Development

The Sum of the Parts

The Joint Council produces a national agenda for research and education

Research Initiatives

A Research Agenda for the State Agricultural Experiment Stations

The Experiment Station Committee on Organization and Policy
The Cooperative State Research Service

Other publications in this series:

Research 1984: The State Agricultural Experiment Stations
Research Perspectives: Proceedings of the Symposium on the Research Agenda for the State Agricultural Experiment Stations

Research Initiatives
1986

ESCOP 86.1

Texas Agricultural Experiment Station
The Texas A&M University System
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Foreword

The Cooperative State Research Service provided partial support for the ESCOP symposium and for publication of the resulting proceedings, as well as for the planning workshop and its related activities. In addition, Dr. J. P. Jordan, Administrator of CSRS, and the CSRS faculty participated in both the symposium and the workshop. ESCOP wishes to thank CSRS for their fine effort.

The following are members of the ESCOP Planning Subcommittee:

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Special thanks are extended to Jim Halpin, Southern Director-At-Large, who functioned as Executive Vice Chairman of the ESCOP Planning Subcommittee during 1984-85. Dr. Halpin helped organize and execute the several meetings.

Dr. Charles Laughlin of the Georgia Agricultural Experiment Station made meeting arrangements for the planning workshop held in Atlanta in August 1985. He arranged for the "near real time" word processing capability that greatly facilitated the preparation of work papers. ESCOP also wishes to thank LeeAnn Spears and Betty Roberts of the Georgia station, and Martha Ford from the Mississippi station, who operated the word processing equipment.

Rodney Foil was chairman of the planning workshop and was responsible for gathering the inputs from industry, farm organizations, professional and disciplinary societies, and the Joint Council. He, along with Dr. Bobby Eddleman and Jason Leng of the Mississippi Agricultural and Forestry Experiment Station, organized the information by means of a microcomputer program and file. Dr. Foil was a major contributor to the resources section of the plan.

Special thanks are due to Tim Shaunty, Kim Haight, and Janet Frye of the Texas Agricultural Experiment Station. Tim Shaunty organized support for the Atlanta workshop and the preceding Woodlands conference and edited much of the draft material for the report. Kim Haight provided excellent staff support throughout the planning process. Janet Frye was editor for the symposium proceedings and helped finalize the planning documentation.

Thanks are also expressed to all members of the affiliate groups and the Special Initiatives Subcommittee, the invited members of the workshop, the members of ESCOP who participated and provided advice throughout the year, and to those others who participated in the symposium. A listing of the participants in the Atlanta workshop follows.

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Executive Summary

The Experiment Station Committee on Organization and Policy has developed a contemporary statement of the agenda for new research in the State Agricultural Experiment Stations. A broad set of new research initiatives has been enunciated, which represents a consensus of industry and the scientific community about what needs to be done by the State Agricultural Experiment Stations.

This study of new initiatives had very broad input from the SAES community; administrators and scientists from more than 30 states were active participants. The Joint Council process, both at the regional and national levels, was involved in the development of the plan. Input from a large array of industrial and farm groups was sought, received, and used in the process. Inputs also were received and used from most of the major scientific and professional societies whose interests include agriculture.

A national Symposium on the Research Agenda for the State Agricultural Experiment Stations was held in June 1985. It provided broad input from inside and outside the SAES system. Participants included scientists, administrators, senior members of both the administration and the Congress, and participants from the industries related to agriculture. There were participants from more than 40 states, including SAES administrators and members of ESCOP's affiliate groups.

A major workshop was held in Atlanta in August 1985 to consider contributed ideas on the need for new research. Participants in the workshop were SAES administrators and scientists chosen to represent both regional and disciplinary interests, members of the affiliate groups, the ESCOP Special Initiatives Subcommittee, representatives from other federal research agencies, and the full membership of ESCOP. In all, 52 people attended the workshop.

This report is the product of the Atlanta workshop. It portrays 21 major new research initiatives, all of which are regarded as very high priority. These initiatives are rather broad statements of goals needed to bring a balanced infusion of new knowledge and technology to U.S. agriculture. For each major initiative, several specific objectives are defined to show how the initiative can be achieved. In all, there are 99 objectives. In the workshop, many more needs were considered, and only the highest priority were retained in this plan. While all elements of the plan are considered very high priority, the workshop identified the top 25 percent of both the broad initiatives and the research objectives. There was consensus on the priorities chosen.

In consolidating a broad and yet detailed set of needs and opportunities into an overview, it is difficult to portray the dynamics and freshness of the new initiatives. In fact, much of what needs to be done in new research, considered in generic terms, is not new but unfulfilled needs. This plan, as intended from the outset, draws heavily on the current thinking at the regional and national level of the Joint Council process, which carries with it a rich input from every SAES. Thus, in the aggregate, one sees a continuity of thinking from previous to present planning. What is new here is the substantially broader input from industry and the scientific societies, a fresh look at emerging problems and opportunities, and a more quantitative method of stating the resources needed to undertake the new research initiatives. While the perception of relative priorities is generally consistent with previous thinking in the SAES system, the consensus development method used in the Atlanta workshop provided a more systematic vehicle for assessing priorities.

An optimistic estimate was made of the resources needed to undertake the new research initiatives. An iterative process was used to determine the final estimates. First, an attempt was made to define the resources necessary to undertake an individual initiative and its objectives, without regard to the total cost of the plan. Next, an arbitrary decision was made to seek new funding no greater than about 20 percent of the present program. Then the resources to be allocated to each initiative were reevaluated and constrained to fit within an overall 20 percent increase. Decisions were based on inputs from the workshop and all other estimates of relative priority. To indicate the general picture of the results, approximately one-half of the proposed new resources were assigned to the initiatives and objectives in the top 25 percent of the plan. Thus, high priorities were given due consideration, but there was balance in the final result.

An important feature of this plan is the separate identification of resources needed for initial or start-up costs and those needed for continuing effort. There is a renewed interest in the administration and Congress for revitalizing the scientific base; this plan identifies the funds needed for renovation, new construction, and equipment for the various initiatives and objectives. It was stated in early meetings with the USDA that the states would share start-up costs equally; the plan assumes full federal funding of the continuing costs.

The plan identifies a need for 1,481 new scientist years (SY) of effort. The one-time cost is $444 million; the continuing cost is $232 million per year. The average start-up cost per SY is $453,000; the average continuing cost is $155,000 per SY. Obviously, not all elements of the plan can be funded at once. However, the plan provides a framework for the yearly budget process wherein specific initiatives or groups of initiatives can be chosen and developed in more detail. The plan suggests an approach that would phase in the initial costs over at least a 2-year period; the continuing costs, if all elements were funded at once, would be incurred over a 3-year period.

Agriculture faces a short-term survival crisis and a long-term need for new technology that will restore and enhance profitability. The ability to compete successfully in the international marketplace must be retained and expanded. The opportunity to provide an infusion of new technology for agriculture has never been greater than it is today, especially with the possibilities that exist in the sciences basic to agriculture. Thus, opportunity and need intersect at a critical point in the pathway of agriculture. The State Agricultural Experiment Stations, working in cooperation with the other contributors to research, have described an agenda for action that will build on a broad base of ongoing research activity and capability. Implementing this agenda will help to sustain and enhance agriculture as a healthy and robust part of the economy of the United States, and improve agriculture's ability to provide food and fiber of highest quality at affordable prices for the consumer. The new knowledge and technology that will come from implementing this plan will go far towards ensuring the conservation of natural resources and the preservation of environmental quality.

Introduction

The State Agricultural Experiment Stations represent a major part of the total agricultural research capability in the United States today. The state-federal relationships that resulted from the passage of the Hatch Act in 1887 have fostered a network of communication and collaboration and have led to a system of agricultural research which meets the needs of the industry while addressing regional and national concerns. State support for agricultural research has grown since the partnership was formed, but federal research support for the broader initiatives that can be addressed by the State Agricultural Experiment Stations needs to be increased. The benefit of agricultural research has been well documented. The annual rate of return on investment is between 30 and 50 percent per year.

Agriculture in the United States faces some very serious problems. Some of these problems can be reduced through the acquisition of new knowledge and technology; other problems, such as the impact of international monetary policy and domestic farm policy, cannot be solved by research alone. However, these factors must be considered when future agricultural research is planned. Whatever outcome is envisioned with regard to international and domestic policy, there will remain an urgent need for a continuing infusion of new agricultural knowledge and technology for this country. The ability to produce food and fiber at the lowest possible cost will be a necessary feature of farming operations, regardless of the outcome of the present crisis.

While the problems agriculture faces are serious, the opportunities for solving these problems through research are great. Thus, the need for new technology and the opportunity to meet this need through research intersect at a critical time. The State Agricultural Experiment Stations have developed a contemporary statement of their research agenda that can be used in the planning and budgeting process at both the federal and state levels.

The ESCOP Planning Subcommittee

The Experiment Station Committee on Organization and Policy studied the need for developing a planning activity during 1983-84. The full committee discussed alternative approaches for the planning activity, and asked the Special Initiatives Subcommittee to make specific recommendations on developing such a plan. In the fall of 1984, ESCOP established a standing Planning Subcommittee and assigned this committee the task of writing a contemporary statement on the research agenda for the State Agricultural Experiment Stations.

The ESCOP Symposium on the Research Agenda for the State Agricultural Experiment Stations was held in June 1985. Providing input to this symposium were administrators and scientists from the State Agricultural Experiment Stations, senior representatives of the administration and the Congress, representatives from the Users Advisory Board, representatives of key agricultural industries, and constructive critics from both inside and outside the system. The product of the symposium is a proceedings, *Research Perspectives*, published in November 1985. The symposium provided important input to the subsequent planning workshop.

The planning workshop was held in Atlanta in August 1985. This report is the product of that workshop.

Objectives of the Planning Activity

The following specific objectives and guidelines were provided by the parent committee to the Planning Subcommittee of ESCOP:

(1) Build on the regional Joint Council activity;
(2) Assure continuing complementarity with the Joint Council process;
(3) Seek input from users of the products of agricultural research and development;
(4) Seek input from the scientific and disciplinary societies;
(5) Assure the participation of the affiliate groups of ESCOP in the planning activity;

(6) Develop participatory communication with related federal agricultural and forestry research agencies;
(7) Coordinate the ESCOP plan with nonagricultural federal research agencies;
(8) Coordinate the ESCOP plan, in its formative stages, with policy and decision makers.

The Experiment Station Committee on Organization and Policy envisions the following uses of the plan:

(1) To communicate the State Agricultural Experiment Stations' contemporary research agenda;
(2) To provide interactive communication with the Special Initiatives Subcommittee of ESCOP to ensure more detailed analysis of selected key initiatives;
(3) To provide information for the ESCOP Budget Subcommittee to use in its formulation of annual budgets;
(4) To provide direction to the Cooperative State Research Service in the development of its strategic plan;
(5) To interact with the Joint Council process by providing input from the State Agricultural Experiment Stations to the National Agricultural Research Committee; input from the four regional associations would be directed to the Planning Subcommittee of ESCOP for consolidation into a single national agenda for the SAESs;
(6) To communicate and interact with other relevant elements of the federal government;
(7) To provide a national planning reference point for individual directors of the State Agricultural Experiment Stations and the regional associations.

Methods

Figure 1 shows the various sources of the information considered by the ESCOP Planning Subcommittee in developing this document. Through the regional research committees and the parent committee, ESCOP, the State Agricultural Experiment Stations were well represented in the process. In addition, the professional societies with agricultural interests were asked to provide their assessment of research needs and opportunities, and these groups made substantial contributions (Table 1). In the spring of 1985, various agricultural industries were asked to contribute their perceptions of research needs (Table 2). A substantial but incomplete response was received. After the Atlanta workshop, a rough draft of the broad initiatives and objectives was sent both to those industries that had responded and to those that had not. Many industries that did not provide a statement of research needs endorsed various elements of the draft plan and gave positive feedback.

Fig. 1. Inputs to the ESCOP Planning Subcommittee

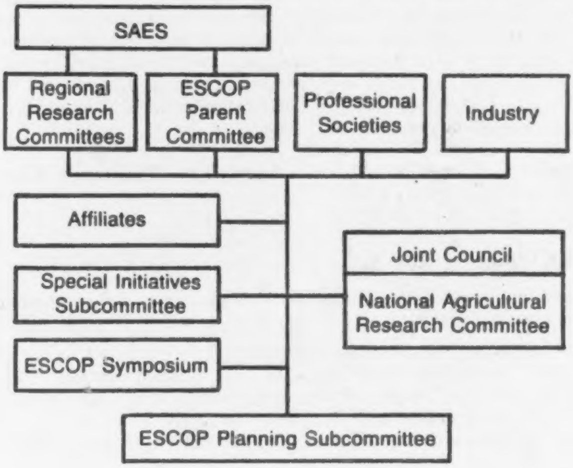

SAES
Regional Research Committees
ESCOP Parent Committee
Professional Societies
Industry
Affiliates
Joint Council
Special Initiatives Subcommittee
National Agricultural Research Committee
ESCOP Symposium
ESCOP Planning Subcommittee

Table 1: Scientific and Professional Organizations

Agricultural Research Institute
American Agricultural Economics Association
American Dairy Science Association
American Home Economics Association
American Phytopathological Society
American Society for Enology and Viticulture
American Society of Agricultural Engineers
American Society of Animal Science
American Society of Biological Chemists
American Society of Plant Physiologists
American Society of Range Management
Association of American Veterinary Medical Colleges
Crop Science Society
Fish and Allied Aquatics Society
National Industry-State Agricultural Research Council
Rural Sociological Society
Society for Industrial Microbiology
Society of Wood Science & Technology
Soil Science Society of America
Weed Science Society of America
Joint Council on Food and Agricultural Sciences
National Agricultural Research Committee
North Central Regional Research Committee
Northeast Regional Research Committee
Southern Regional Research Committee
Western Regional Research Committee
ESCOP Research Planning Symposium
ESCOP Special Initiatives Subcommittee
ESCOP Home Economics Research Subcommittee

Table 2: Industry and Farm Organizations

American Association of Nurserymen
American Farm Bureau
American Forage and Grassland Council
American Meat Science Association
American Seed Trade Association
American Soybean Association
Farm and Industrial Equipment Institute
Forest Farmers Association
Grocery Manufacturers of America
Grain Sorghum Producers
National Institute of Food Technologists
National Agricultural Chemicals Association
National Alliance of Independent Crop Consultants
National Association of Conservation Districts
National Association of Wheat Growers
National Broiler Council
National Cattlemen's Association
National Cotton Council of America
National Grain and Feed Association
National Grange
National Pest Control Association
National Pork Producers Council
National Turkey Federation
Potash and Phosphate Institute
Sheep Industry Development Program
Southeastern Poultry and Egg Association
Southern Industrial Forest Research Council
The Irrigation Association
The Poultry Science Association
U. S. Animal Health Association

The ESCOP affiliates (including Home Economics, Forestry, Veterinary Medicine, and the 1890 institutions) participated actively in the symposium and the Atlanta workshop. The Planning Subcommittee met several times in 1985 with the Special Initiatives Subcommittee to coordinate the activities of these closely related groups. Members of the Special Initiatives Subcommittee participated in both the symposium and the workshop.

The results of the ESCOP symposium were analyzed in detail, and specific elements that pertained to the development of the research initiatives were extracted and provided to participants in the Atlanta workshop.

Because chairmen of the Joint Council regional research committees were also assigned to the ESCOP Planning Subcommittee, maximum use was made of the thinking that had occurred at the regional level when the research initiatives in this plan were defined.

Other federal agencies involved in agricultural and forestry research also participated in the symposium and workshop. The Cooperative Extension Service participated in the symposium and had special presentations made to its Extension Committee on Organization and Policy. Representatives of other federal research agencies have viewed the planning document in draft form, and their comments have been considered in preparing the final document.

The technical aspects of the research initiatives and objectives were developed at the Atlanta workshop.

Prior to the Atlanta meeting, a small group of ESCOP Planning Subcommittee members met at the Woodlands near Houston to digest the large amount of information that had been received initially. They developed a minicomputer program that allowed for easy sorting and retrieving of information and developed a "straw man" document that arranged all of the contributions from the scientific and industrial community in a format that could be more easily used by the technical planning group.

Those who attended the planning workshop in Atlanta were selected to represent both regional and disciplinary areas of interest. The full committee of ESCOP was invited to participate, as was the Special Initiatives Subcommittee. Members of the Planning Subcommittee led a number of working groups within the workshop. Other scientists and administrators were invited, so that there would be complete regional and disciplinary balance in the process. The chairmen of both the 1987 and 1988 Budget Subcommittees of ESCOP were participants, as were representatives from all of the affiliate groups and related federal agencies.

First, members of the workshop agreed upon a format and operating procedures for developing the final documentation. Then, four subcommittees were established to address the areas of:

1. Natural Resources
2. Animal Sciences
3. Plant Sciences
4. Social Sciences

These groups were asked to consider, but not be limited by, the information that had been presented to them. They were asked to develop major new research initiatives. Specifically, they were instructed not to justify or defend the base program but to determine research needs in addition to the base program. For each of these research initiatives, specific research objectives were defined to show what research must be undertaken to meet the stated goals.

As part of the process, the four subcommittees were asked to evaluate the relative priorities of the initiatives they developed, considering both the need for the research and the opportunity to achieve meaningful results. They were also asked to prioritize the specific research objectives outlined for each initiative.

When the four subcommittees had completed their work, the chairmen met to consider all of the initiatives and objectives. Through an interactive process, the subcommittee chairmen developed a list of 21 initiatives, representing the highest priority needs of U. S. agriculture and the areas with the greatest potential for achieving applicable results from research. To carry out those research initiatives, 99 research objectives were identified. Each initiative had between two and nine research objectives. The chairmen of the four subcommittees then reached a consensus on the top 25 percent of the initiatives and the top 25 percent of the individual objectives, in terms of their relative priority.

For each of the major initiatives identified, the following documentation is provided in this plan: (1) definition of the research initiative; (2) justification of the need for the initiative; (3) a brief statement of the research objectives necessary to accomplish the initiative; (4) a description of the relationship of this new research to the existing base program; (5) a list of the advocates for the new initiative, including industrial and scientific groups; and (6) resources necessary to implement each of the research objectives, including one-time or start-up funding and continuing or annual costs.

The statements of resource needs were based on information from the technical workshop, from the ESCOP Special Initiatives Subcommittee, from recent institutional surveys, and from the Joint Council (including inputs from the National Agricultural Research Committee). All of the initiatives were given very high priority by the workshop participants. However, in allocating overall resources, preference was given to the highest priority needs—those which fell among the top 25 percent of the initiatives and specific research objectives identified by the workshop.

An interactive process was used to develop the resources needed for each research objective, and from that, the aggregate resources needed to accomplish each initiative. At first an attempt was made to define the optimum resources for each initiative, regardless of the total cost of the plan. Subsequently, an arbitrary decision was made to limit the total resources in the plan to approximately 20 percent more than existing programs. The allocation to each initiative was then reevaluated and constrained to fit within an overall 20 percent increase.

To more realistically assess the resources needed to implement this plan, three levels of resource needs were identified: high, average, and low. Table 3 shows the estimates of costs associated with one scientist year of activity for each of these three categories. Dollars for both the start-up and the continuing costs are identified. Start-up costs for a high cost (i.e., high technology) research activity were assumed to be $425,000 per SY; for an average cost scientist year of activity, $225,000; and for lower cost scientific endeavors, $150,000. Continuing annual operating costs of $180,000, $150,000, and $120,000 for a high, medium, and low cost scientist year of activity were assumed.

Resources were distributed to the 21 new research initiatives using subjective judgment and the inputs mentioned above. The SYs were then distributed to the specific research objectives under each of the new initiatives. Using the cost model described above, the cost for each of the objectives was determined and the aggregate of these established the total cost of the research initiative.

Table 3. Resource Needs For Objectives (Per Scientist Year)

		($ x 1000)	
	High	Average	Low
Start-up Costs			
Facilities	225	150	100
Equipment	200	75	50
Total	425	225	150
Continuing Costs			
Personnel	120	100	80
Operating Expenses	36	30	24
Equipment	24	20	16
Total	180	150	120

Results

This process resulted in a statement of need for 1,481 new SYs of activity in the State Agricultural Experiment Stations. The one-time cost for implementing this plan is $444 million. Continuing annual costs are $232 million per year (Table 4). The average first year cost, including facilities and equipment per scientist year, is $453,000. The average continuing cost per scientist year is $155,000. Allocations were based on the relative priorities developed in the workshop, and one-half of the total resources identified was assigned to the top 25 percent of the research initiatives and objectives. Although research priorities had a substantial impact on the assignment of resources, the balance of the total program was maintained.

Figure 2 shows the investment strategy proposed for implementing the total plan in the shortest possible time. A 2-year phase-in of the one-time costs is proposed, with the states sharing these costs equally. A 3-year phase-in of the continuing program is proposed, with the total cost coming from federal funds. If this entire plan were implemented in the immediate future, the federal funds required for the first year would be $188 million; for the second year, $265 million; and for the third and following years, $231 million.

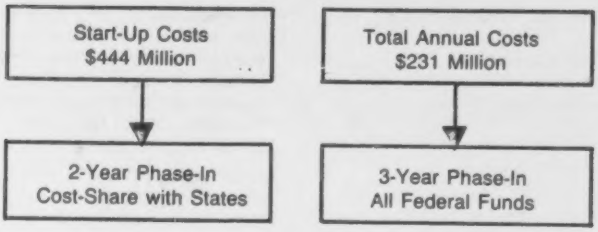
Table 5 is a summary of the broad research initiatives and the specific research objectives selected during the Atlanta workshop. The top 25 percent of each are designated with an asterisk (*).

Table 4: Resource Needs by Initiative

INITIATIVE	FUNDS (THOUSANDS)	
	ONE-TIME	CONTINUING
Natural Resources		
*Sustaining Soil Productivity	$18,750	$11,951
*Maintaining and Protecting Water Quality and Quantity	48,225	25,440
Forest Profitability	12,875	7,650
Rangelands and Pasturelands	7,500	6,000
Effects of Atmospheric Deposition	4,600	2,790
Animal Sciences		
*Biological Efficiency of Animals	31,875	13,500
Animal Health and Disease	42,500	18,000
Plant Sciences		
Genetic Improvement of Plants	13,875	9,750
Improved Management of Crop Pests and Disease	11,250	7,500
Agriculture in the Urban Environment	7,500	6,000
Economic and Social Concerns		
Short-Term Adjustments for Enhancing Economics of Agriculture	9,000	7,200
*Agricultural Policy in a Global Setting	7,500	6,000
Market Penetration and Efficient Marketing of Agricultural and Forestry Products	3,000	2,400
Rural Family and Community Well-Being	7,500	6,000
*Interrelationships of Food and the Nutritional and Health Status of People	31,750	16,200
Multidisciplinary Issues		
*Biotechnology	91,375	38,700
Computer Technology for Agricultural Management	21,250	9,000
Robotics in Agriculture	10,625	4,500
Processing and Quality Enhancement	42,500	18,000
Energy Efficient Systems	5,400	3,060
*Integrating Agricultural Technology	15,000	12,000
Total	**$443,850**	**$231,641**

(*denotes top 25 percent of new research initiatives)

Fig. 2. Investment Strategy



FEDERAL FUNDS
(Millions)

Year	Start-Up	Annual	Total
1	$111	$ 77	$188
2	111	154	265
3	—	231	231

Table 5: New Initiatives and Objectives

***Sustaining Soil Productivity**
  *Erosion-Soil Property Relationships
  Soil Conservation Policy
  Soil Conservation Economics
  Status of Soil Productivity
  *Tillage Management Interactions

***Maintaining and Protecting Water Quality and Quantity**
  *Groundwater Quality and Quantity
  *Water Use Efficiency
  Chemical and Sediment Movement
  *Water Yield
  Conservation Practices
  Use Policy and Economics

Forest Profitability
  *Silvicultural and Protection Techniques to Increase Yields of Timber
  Techniques for Producing Superior Tree Varieties
  New Manufacturing and Processing Technologies
  New Products and New Uses for Wood

Rangelands and Pasturelands
  Rangeland Ecology
  Plant/Animal Interactions
  Water Management
  Plant Improvement
  Management Systems
  Weed and Brush Management

Effects of Atmospheric Deposition on Crops, Forests, Livestock, Wildlife and Associated Ecosystems
  Determine Chemical Exposures
  Pollutant-Stress Relationships
  Dose-Response Relationships
  Intensive Field Investigations
  Effects of Toxic Metals

***Biological Efficiency of Animals**
  *Animal Genetics
  *Reproductive Physiology
  Animal Nutrition
  Animal Protein and Lipid Synthesis

Animal Health and Disease
  *Immunological Advances
  Integrated Health Management
  Animal Care
  *Embryo Transfer

Genetic Improvement of Economically Important Plants
  *Genetics and Breeding
  Pest Resistance
  Environmental Stress
  Physiological and Morphological Parameters
  Soil Microorganisms

Table 5: New Initiatives and Objectives (continued)

Improved Management of Crop Pests and Diseases
- *Incidence, Prediction, Biocontrol
- Biocide Management
- Quantifying Constraints to Plant Productivity
- Epidemiological Systems
- Biological Control Techniques
- Integration of Pest and Disease Management Into Crop Production Systems

Agriculture in the Urban Environment
- Plant Materials for Non-Traditional Environments
- Maintenance Strategies for Plants in Diverse Environments

Short-Term Adjustments for Enhancing the Economics of Agriculture
- *Lower Unit Production Costs
- Improved Risk Management
- Alternative Financial Management Strategies
- Resource and Social Implications of Adjustments

***Agricultural Policy in a Global Setting**
- Commodity, Factor, and Financial Market Relationships
- Political Economy of Domestic and Foreign Commodity Policy
- *Comparative Productivity Growth and Competition in World Markets
- Impacts of Technological Change
- *Policy and Institutional Design

Market Penetration and Efficient Marketing of Agricultural and Forest Products
- Supply, Demand, and Price Relationships
- Grades and Standards
- Market Efficiency and Performance
- International Market Development
- Market Strategies and Power

Rural Family and Community Well-Being
- *Family Stress Factors
- Environmental and Safety Factors for Families
- Economic Alternatives for Non-Metropolitan Areas
- Organizing Capacities of Communities
- Future Governance Systems for Non-Metropolitan Areas

***Interrelationships of Food and the Nutritional and Health Status of People**
- Human Nutritional Requirements
- Dietary Practices
- Nutritional Quality of Foodstuffs
- Bioavailability of Nutrients
- *Dietary Health Risks

***Biotechnology**
- Plant Productivity
- Plant Disease Resistance
- Nutritional Quality of Plants
- *Biological Control of Pests
- *Biologically Active Materials
- *Diagnostic and Immunologic Products
- Animal Disease Resistance
- *Animal Development and Productivity
- *Environmental Impact of Biotechnology

Computer Technology for Agricultural Management
- *Production Management/Decision Models
- Farm-Related Cost Accounting Models
- Expert Systems for Management/Decision Making
- Price and Income Policies and Foreign Trade

Robotics in Agriculture
- *Basic Research for Sensor Technology Development
- Electronic Systems for Plant Production
- Electronic Systems for Animal Production
- Electronic Systems for Food Processing

Processing and Quality Enhancement
- Processing
- Biotechnology
- Food Safety
- By-Products and the Environment

Energy Efficient Systems
- Efficient Plant and Animal Production and Processing Systems
- Conversion Technology
- Energy Assessment

***Integrating Agricultural Technology**
- *Assessment of New Production Technologies
- Impacts of Market Forces on Enterprise Profitability
- Capital Investment and Financial Requirements
- Develop Integrated Farming Systems

(*denotes top 25 percent in priority)

New Initiatives

SUSTAINING SOIL PRODUCTIVITY

RESEARCH INITIATIVE

To develop economically feasible soil management systems that protect the soil resource while reducing the need for inputs to maintain productivity.

JUSTIFICATION

The soil resource is a primary determinant in the production of food and fiber. Wind and water erosion leads to significant losses of organic matter and environmental degradation, and threatens the long-term productivity of cropland and large portions of range and forest lands. Yet, in an effort to minimize production costs and maximize economic stability, farmers have begun using production methods, tillage practices, and equipment that contribute to soil erosion and declining soil productivity. Though data are available on the distribution, kinds, and properties of soils, data bases have not been developed to assess quantitatively the effects of different cropping systems on basic soil properties affecting productivity. Tillage systems such as no-till and minimum tillage could reduce soil erosion and sustain or even enhance productivity. But there is a serious lack of knowledge about the long-term success of these practices in regards to the interaction of soil type, plant variety, fertilizer needs and practices, equipment, planting and harvesting practices, cropping sequences, and pest management.

OBJECTIVES

1. **Erosion-Soil Property Relationships.** The effects of erosion on chemical, physical, and biological properties of soil will be identified in order to develop soil productivity indices.

2. **Soil Conservation Policy.** Measuring the ways alternative policies affect the adoption of soil conservation practices and technologies on farms of various sizes will provide legislators and regulatory agencies with data on which to base policy decisions.

3. **Soil Conservation Economics.** Evaluating alternative soil erosion control practices will provide farmers with appropriate choices for their particular operations.

4. **Status of Soil Productivity.** Methods for developing and utilizing soils information must be established in order to evaluate the regional and national impacts of different management systems on the quality of the soil resource.

5. **Tillage Management Interactions.** Research is needed to define the interrelationships among tillage practices and other components of the production system—including soil type, crop rotation and sequences, planting and harvesting methods, varieties, fertilizer practices, and pest management—in order to develop economical and soil-conserving crop production systems.

RELATIONSHIP TO BASE PROGRAM

Proposed research will focus in part on determining the interrelationships between the chemical, physical, and biological properties of soils and the loss of soil due to erosion that, in turn, affect the productive potential of soils. Past research on identifying and evaluating individual soil properties or processes has generated valuable data which will be used to augment the proposed studies. These studies must evaluate the impacts of many soil and plant management systems in different climates and soil types, and clearly distinguish between changes in production and changes in potential productivity of soils, which past research has failed to do.

New thrusts on tillage management research will produce and make available information on the many interactions of soil, climate, crop sequence, varieties, equipment, fertilizer, and pest management, with appropriate economic interpretation. This badly needed information will complement limited current research and dispel farmer concerns and doubts about the practicability of these conservation practices. In addition, new research will evaluate the impacts of conservation policy on the adoption of new practices and on the economic well-being of producers who use such practices.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Farm Bureau
American Soybean Association
Farm and Industrial Equipment Institute
National Grange
Soil Science Society of America
North Central Agricultural Research Committee
Northeastern Agricultural Research Committee
Southern Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences
ESCOP Symposium

Sustaining Soil Productivity

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Erosion-Soil Property Relationships	8	$ 4,800	$2,400	$2,036	$ 815	$ 400	$10,451
Soil Conservation Policy	6	900	450	600	180	120	2,250
Soil Conservation Economics	6	900	450	600	180	120	2,250
Status of Soil Productivity	6	900	450	600	180	120	2,250
Tillage Management Interactions	50	5,000	2,500	4,000	1,200	800	13,500
SUBTOTALS		$12,500	$6,250	$7,836	$2,555	$1,560	
TOTALS	76	$18,750		$11,951			$30,701

MAINTAINING AND PROTECTING WATER QUALITY AND QUANTITY

RESEARCH INITIATIVE

To provide an adequate quantity and acceptable quality of water while sustaining agricultural, industrial, and municipal activities.

JUSTIFICATION

Water is a significant factor in the cost of production, and both the quantity and quality of water available for agriculture are declining. Approximately 86 percent of the total water resource in the continental United States is in groundwater aquifers, and dependence on this resource is increasing. Agriculture uses 68 percent of the groundwater withdrawn, and half the U.S. population depends on groundwater for drinking water. In addition, research is needed on contamination of surface water as well as sediment and chemical movement. The protection and efficient use of our water resources are crucial to both agricultural production and economic stability. The key will be developing integrated systems for conserving water and finding new sources.

OBJECTIVES

1. **Groundwater Quality and Quantity.** Methods must be developed for predicting the environmental fate of agricultural chemicals and assessing the risks of groundwater contamination in order to develop data on which to base regulatory guidelines.

2. **Water Use Efficiency.** Economical water management and irrigation systems must be developed to provide farmers with alternatives that minimize water quality degradation.

3. **Chemical and Sediment Movement.** The effects of surface and subsurface flow on the transport of sediments and chemicals must be determined to provide data for developing regulatory guidelines.

4. **Water Yield.** The development of strategies and practices such as remote sensing will increase total water yield and availability for both irrigated and nonirrigated lands.

5. **Conservation Practices.** Integrated systems and procedures must be developed—including economical, nonchemical, crop production practices and new varieties and species of crops—that will conserve existing water supplies and prevent soil erosion.

6. **Use Policy and Economics.** Procedures must be developed to provide information necessary for the allocation, efficient use, realistic pricing, and equitable distribution of water resources.

RELATIONSHIP TO BASE PROGRAM

Base program funding has led to the development of agricultural practices that limit the use of persistent chemicals, reduce soil erosion, and eliminate the wasteful loss of irrigation water. However, integration of these individual practices into farming systems that are both productive and environmentally sound is a much more complex task requiring resources beyond those available in the base program. Farming systems research will require that fundamentally new disciplines such as knowledge engineering and agricultural biotechnology be brought to bear on problems of agricultural production. For example, advanced irrigation technologies must be integrated with conservation tillage where possible to make efficient use of the total water resource while preserving environmental quality. Past research efforts have concentrated on preventing degradation of surface water quality by agricultural enterprises. Because of the long transit time for chemicals moving in soils, complementary programs are needed to address the problems of groundwater pollution now appearing throughout the nation. Special efforts are needed to reclaim aquifers where water quality is already impaired. This new initiative will allow the hiring of trained scientists and will provide modern facilities to ensure the rapid diffusion of new technology and its products into existing programs. Additional emphasis in the social sciences is also needed to develop policy alternatives which will ease the transition expected to occur as agricultural water supplies are reallocated among competing users. The integration of social and physical science research efforts should speed the adoption of many new technologies to ensure that adequate supplies of high quality water continue to be available for all users.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Soybean Association
National Alliance of Independent Crop Consultants
American Society of Agricultural Engineers
Soil Science Society of America
North Central Agricultural Research Committee
Northeastern Agricultural Research Committee
Southern Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
ESCOP Symposium

Maintaining and Protecting Water Quality and Quantity

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Groundwater Quality and Quantity	46	$10,350	$ 9,200	$ 5,520	$1,656	$1,104	$27,830
Water Use Efficiency	35	5,250	2,625	3,500	1,050	700	13,125
Chemical and Sediment Movement	23	5,175	4,600	2,760	828	552	13,915
Water Yield	35	5,250	2,625	3,500	1,050	700	13,125
Conservation Practices	11	1,100	550	880	264	176	2,970
Use Policy and Economics	10	1,000	500	800	240	160	2,700
SUBTOTALS		$28,125	$20,100	$16,960	$5,088	$3,392	
TOTALS	160	$48,225		$25,440			$73,665

FOREST PROFITABILITY

RESEARCH INITIATIVE

To increase the profitability of forest lands and forest products by improving regeneration techniques, pest management systems, cultural practices, harvesting methods, forest product utilization, manufacturing processes, and domestic and foreign markets.

JUSTIFICATION

Global demand for lumber, plywood, particleboard, and paper is expected to expand 60 percent by the year 2000. Similar projections have been made with regard to alternate uses of U.S. forests such as tourism and recreation. The keys to future expansion will be maintaining the health and vitality of forest resources, developing new products and processing techniques, and cultivating world markets. Consumer costs need to be kept low and export opportunities met without jeopardizing other important values of the forests.

OBJECTIVES

1. **Silvicultural and Protection Techniques to Increase Yields of Timber.** More efficient forest management practices, such as seed production, stand establishment (particularly regeneration), harvesting, pest management, and fire protection, must be developed to reduce production costs and increase yields.

2. **Techniques for Producing Superior Tree Varieties.** Advances in biotechnology will make it possible to significantly shorten the cycle in breeding superior tree varieties that are more productive and resistant to insects and diseases, thereby reducing input costs and increasing profitability.

3. **New Manufacturing and Processing Technologies.** The market for forest products can be improved by developing innovative systems for automatically designing and manufacturing high-quality, reasonably priced residential structures and furniture.

4. **New Products and New Uses for Wood.** Developing ways to improve the properties and performance of wood products and composites, particularly those made from low quality hardwoods, will expand domestic and foreign markets.

5. **Price and Income Policies and Foreign Trade.** Establishing long-term anticipatory policies on the supply and use of forest products, import demands, trade barriers, and changing export marketing practices will help ensure stability in international trade and put the United States in a position to become a net exporter of wood and wood products.

RELATIONSHIP TO BASE PROGRAM

The base program of forestry research emphasizes the culture of trees and forests, genetic improvement, interrelationships between timber and other forest resources, fundamental properties of wood, and processing technologies which enhance the value of wood. However, most efforts lack the manpower and supporting resources needed to make significant progress with existing technologies. For example, the domestication of trees is in its infancy. Little effort is devoted to understanding the biological, physical, and chemical processes which are critical to improving forest productivity and wood utilization. Emerging technologies in cell and plant biology, pest management, automation, and wood chemistry must be exploited to ensure adequate supplies of forest products at competitive prices and make the U.S. a net exporter of wood products. The exploitation of these technologies will require more emphasis on the sciences basic to forestry, increased multidisciplinary efforts, and much improved laboratories. Providing this emphasis through the new initiatives will also strengthen the existing base program in forestry research.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

Forest Farmers Association
Society of Wood Science and Technology
Northeastern Agricultural Research Committee
Southern Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences

Forest Profitability

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Silvicultural and Protection Techniques to Increase Yields of Timber	25	$3,750	$1,875	$2,500	$ 750	$ 500	$9,375
Techniques for Producing Superior Tree Varieties	10	1,500	750	1,000	300	200	3,750
New Manufacturing and Processing Technologies	5	1,125	1,000	600	180	120	3,025
New Products and New Uses for Wood	5	1,125	1,000	600	180	120	3,025
Price and Income Policies and Foreign Trade	5	500	250	400	120	80	1,350
SUBTOTALS		$8,000	4,875	$5,100	$1,530	$1,020	
TOTALS	50	$12,875		$7,650			$20,525

RANGELANDS AND PASTURELANDS

RESEARCH INITIATIVE

To develop improved and profitable multiuse management systems specifically for Western rangelands, native pastures, and humid grazing lands.

JUSTIFICATION

Rangelands and pasturelands comprise more than 70 percent of the land area of the United States. Most rangeland is in the arid Western states, while the more humid East supports a large livestock industry on native pastures and intensively managed tame pastures. These grazing lands provide water, wildlife, scenic beauty, and recreation, in addition to feed for livestock. In some areas of the South and Southwest, net income from recreational uses exceeds that from livestock. Water conservation and yields in many aquifers could not be sustained without the vegetative cover of rangelands and pasturelands. Humid grasslands have unexploited potential as a source of feed for grazing livestock and wildlife, and for mechanical harvesting. The use of more efficient production systems on marginal and erosive lands could make grazing or forage production competitive with crop production, and reduce the likelihood of serious soil erosion which would result if these soils were cropped. Despite the potential for increased productivity of pastureland and rangeland, the technology for managing these lands lags far behind that used in more traditional agricultural systems. Innovative vegetation and animal management systems can significantly increase the production efficiency on U. S. rangelands and pasturelands.

OBJECTIVES

1. **Rangeland Ecology.** Fundamental ecological data must be gathered for range and pasture plants and communities, and biological and ecological concepts applicable to multiuse management of rangelands and pasturelands must be identified.

2. **Plant/Animal Interactions.** Determining the effects of both domestic and wild grazing animals on the morphological development and physiological functions of range and pasture plants and communities will lead to the development of grazing strategies for domestic and wild animals.

3. **Water Management.** Determining how grazing and other uses affect water balance, and what practices are best for efficiently using water on- and off-site and for controlling erosion, will enhance the use of rangelands and pasturelands.

4. **Plant Improvement.** Developing more biologically efficient herbaceous or woody species will improve forage production, wildlife habitat, and soil and water conservation on rangelands and pasturelands.

5. **Management Systems.** Decision-making aids and information systems must be developed for users of rangeland and pastureland to help them manage these resources.

6. **Weed and Brush Management.** The development of reliable, economical, safe methods of managing unwanted plants on rangeland and pastureland will improve production, reduce erosion, and reduce animal death from poisonous plants.

RELATIONSHIP TO BASE PROGRAM

This new initiative will enhance the base program by adding basic ecological research and filling gaps in the existing program. The new objectives in ecology, plant/animal interactions, water management, and management systems will promote a systems approach in research on the multiple use of rangelands and pasturelands, and expand the horizons of traditional research. The proposed increases in plant improvement and weed and brush management would add to underfunded and understaffed efforts in these areas, and allow the development of problem-solving teams where now only projects exist. Many SAESs lack balanced range and pasture programs due to shortages of resources. Funding of the new initiatives will add new expertise and resources to base programs and strengthen overall efforts.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

National Cattlemen's Association
National Agricultural Research Committee
American Forage and Grassland Council
Society for Range Management
ESCOP Symposium
Soil Science Society of America
USDA Joint Council on Food and Agricultural Sciences

Rangelands and Pasturelands

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Rangeland Ecology	20	$1,000	$2,000	$1,600	$ 480	$320	$5,400
Plant/Animal Interactions	10	500	1,000	800	240	160	2,700
Water Management	5	250	500	400	120	80	1,350
Plant Improvement	5	250	500	400	120	80	1,350
Management Systems	5	250	500	400	120	80	1,350
Weed and Brush Management	5	250	500	400	120	80	1,350
SUBTOTALS		$2,500	$5,000	$4,000	$1,200	$800	
TOTALS	50	$7,500		$6,000			$13,500

EFFECTS OF ATMOSPHERIC DEPOSITION ON CROPS, FORESTS, LIVESTOCK, WILDLIFE, AND ASSOCIATED ECOSYSTEMS

RESEARCH INITIATIVE

To determine the beneficial and/or harmful effects of airborne chemicals (sulfur and nitrogen oxides, volatile organic chemicals, carbon dioxide, toxic metals, and their transformation products) on the health and productivity of crops, forests, livestock, wildlife, and associated ecosystems.

JUSTIFICATION

Many human activities are changing the chemistry of the atmosphere. The beneficial and/or harmful effects of these changes are major scientific and policy issues in many parts of the United States. The National Acid Precipitation Program (NAPAP) funds an $85 million research program each year, which focuses on measuring regional emissions and deposition of airborne chemicals, and their effects on forests and water. There is little research on agricultural crops. Increased research is needed to determine if, when, where, and to what extent airborne chemicals are changing the productivity and quality of forests, crops, livestock, wildlife, and surface waters. The results can provide a basis for developing alternative management strategies, and can guide public policy debates.

OBJECTIVES

1. **Determine Chemical Exposures.** Information on the chemical compositions of rain and snow and the dry deposition of airborne chemicals (gases and aerosols) will be gathered throughout the United States to pinpoint areas of greatest risk under ambient conditions.

2. **Pollutant-Stress Relationships.** Genetic and cultural practices for mitigating the harmful effects of air pollutants will be developed by studying the relationships between pollutants and stress factors (such as drought, frost, insects, and disease), and the effects of pollutants on biological processes such as photosynthesis and nutrient uptake.

3. **Dose-Response Relationships.** Determining the response of crops and forest trees to variable doses of major pollutants, the short- and long-term effects of exposure, and the effects of pollutants when combined with other stress factors, will help researchers assess the potential of new plant breeding strategies.

4. **Intensive Field Investigations.** Multidisciplinary research teams working with continuous atmospheric monitoring systems will determine chronic and acute effects of pollutants on different physiological and ecological processes in crop and forest ecosystems, and learn to predict plant responses to ambient environmental conditions.

5. **Effects of Toxic Metals.** This research will determine if, when, and where certain toxic metals and other biologically active airborne chemicals accumulate in food crops, fish, feed grains, and forage for livestock and wildlife.

RELATIONSHIP TO BASE PROGRAM

The new initiative augments the base program on atmospheric deposition, which does not completely address problems of increasing importance to agriculture and society. The interaction of atmospheric toxicants on elements of the agroecosystem, the cumulative effects on micro- and macroflora, and the impact of atmospheric deposition on environments under stress are areas of emphasis not included in the base program. With this new initiative, the design of genetic and physiological marker systems to overcome the effects of atmospheric deposition, especially in plants, can be investigated. This should prove very valuable in designing new biota for agricultural uses.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

Forest Farmers Association
Southern Regional Committee
Experiment Station Committee on Organization and Policy
National Agricultural Research Committee
National Grange
Southern Industrial Forest Research Council

Atmospheric Deposition

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Determine Chemical Exposures	6	$ 600	$ 300	$ 480	$144	$ 96	$1,620
Pollutant-Stress Relationships	3	675	600	360	108	72	1,815
Dose Response Relationships	3	450	225	300	90	60	1,125
Intensive Field Investigations	6	600	300	480	144	96	1,620
Effects of Toxic Metals	2	450	400	240	72	48	1,210
SUBTOTALS		$2,775	$1,825	$1,860	$558	$372	
TOTALS	20	$4,600		$2,790			$7,390

BIOLOGICAL EFFICIENCY OF ANIMALS

RESEARCH INITIATIVE

To increase the biological efficiency of animals and the nutritive quality of animal products by improving the genetics, diets, housing, and handling of animals.

JUSTIFICATION

Animal products are major sources of many essential nutrients in the human diet. Animal protein, for example, is a high-quality, complete, balanced protein. But the composition (such as muscle-to-fat ratio) of certain traditional animal products must be altered to make them more acceptable to consumers in light of current dietary standards. The quality of animal products for human consumption can be increased by maximizing animal efficiency, manipulating digestive tract microflora and other physiological processes to optimize feed and forage utilization, and controlling the cellular mechanisms responsible for the synthesis of animal proteins and lipids so that more usable protein is produced per unit of feed.

OBJECTIVES

1. **Animal Genetics.** Genetic engineering techniques will be developed to produce in vivo or in vitro nutritional or growth promotants, or permit genomic insertions or alterations that increase biological efficiency. The efficiency of conventional breeding techniques will be increased by making selection more precise and by augmenting it with gamete or embryo selection, multiplication, and storage.

2. **Reproductive Physiology.** Reproductive efficiency will be increased by improving control of environmental and physiological factors adversely affecting estrus, ovulation, fertilization, and embryonic and perinatal survival of young.
The development of improved techniques for embryo and gamete multiplication, sexing, storage and transfer will increase the contribution of superior germplasm.

3. **Animal Nutrition.** The choice and use of feedstuffs by animals can be improved by: a) identifying and manipulating factors which increase intake and nutrient availability; b) developing techniques to manipulate rumen microflora to enhance their utilization of cellulose and starch; c) identifying processes to improve the availability, digestibility, and metabolic interactions of minerals; and d) developing more precise nutrient requirements for animal growth and reproduction under various genetic and environmental conditions.

4. **Animal Protein and Lipid Synthesis.** Animal products can be improved by altering the physiologic mechanisms that control the synthesis of protein and fat. This can be accomplished by: a) identifying the mechanisms that control synthesis and degradation of protein and fat in food animals; b) identifying the factors that control muscle and fat cell number; and c) seeking genetic, nutritional, or endocrinological controls over these mechanisms.

RELATIONSHIP TO BASE PROGRAM

Because of restricted base funding, current research is unable to apply much of the new technology which has the potential to greatly improve the genetic and reproductive capacity of animals and to improve the quality and quantity of animal products. Only a few laboratories are now equipped to apply the new molecular biology techniques to animal research. This new funding initiative will permit the equipping of laboratories and the hiring of trained researchers needed in this area. Current research is largely confined to gross measurements of performance, with very limited study of the basic cellular mechanisms responsible for synthesis of animal protein and lipids and the factors influencing efficiency of feed utilization for these processes. As the new technology is applied, researchers will be able to economically evaluate the effects of genetic and physiological change on production, marketing, processing, and consumer acceptance. Producers are in critical need of such technology. An efficient animal production system is required to assure an adequate supply of animal products for human health.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

National Cattlemen's Association
American Dairy Association
American Society of Animal Science
National Pork Producers
Southern Regional Research Committee
Western Agricultural Research Committee
American Forage and Grasslands Council
National Broiler Council
American Meat Science Association
North Central Research Planning Committee
ESCOP Symposium

Biological Efficiency of Animals

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Animal Genetics	20	$ 4,500	$ 4,000	$2,400	$ 720	$ 480	$12,100
Reproductive Physiology	20	4,500	4,000	2,400	720	480	12,100
Animal Nutrition	20	4,500	4,000	2,400	720	480	12,100
Animal Protein and Lipid Synthesis	15	3,375	3,000	1,800	540	360	9,075
SUBTOTALS		$16,875	$15,000	$9,000	$2,700	$1,800	
TOTALS	75	$31,875		$13,500			$45,375

ANIMAL HEALTH AND DISEASE

RESEARCH INITIATIVE

To reduce disease losses of livestock, poultry, and fish in order to maintain an affordable, high quality food supply and to sustain a profitable food animal industry. Biotechnology offers unprecedented opportunities for accomplishing these challenging goals.

JUSTIFICATION

Animal diseases cost the livestock industry an estimated 20 percent of its annual gross income. In 1983 this loss, which is ultimately borne by the consumer, amounted to an estimated $17.5 billion. These losses must be reduced in order to improve the profitability of livestock agriculture and to improve the quality of the human diet. More than 150 diseases of animals are transmissible to humans, according to the World Health Organization. Control or eradication of these diseases improves and protects the health of the human population. With animal production systems being intensified, there is greater need for integrated animal health programs.

OBJECTIVES

1. **Immunological Advances.** Improved immunological techniques must be developed for preventing animal diseases. There also is a need for early and rapid detection of diseases and organism contamination of animal products.

2. **Integrated Health Management.** The total livestock health management system will be defined as it pertains to the profitability of the livestock operation, in order to determine the incidence and economic impact of diseases and evaluate the effectiveness of disease control methods.

3. **Animal Care.** Techniques for the care and maintenance of experimental and production animals will be refined, with special emphasis on factors that minimize stress and promote animal well-being.

4. **Embryo Transfer.** Procedures for embryo preservation and transfer and for genetic manipulation will be improved. Methods of determining potential disease transmission via embryos will be developed so that embryos can be certified as disease free.

RELATIONSHIP TO BASE PROGRAM

This new initiative will permit the extensive use of new medical research technology which is available but, because of restricted base funding, is being used only to a limited extent in solving animal health problems. The application of such technology will permit breakthroughs on complex animal health problems not possible under the current program. There is almost no research on the role of embryos in animal disease transmission or on the care and stress-free maintenance of laboratory and production animals. These new efforts will provide information critical to disease prevention, to establishing standards of animal care, and to answering questions posed by an increasingly concerned public. Current research is exploring various aspects of disease control, but very limited work is in progress on the economic value of disease control procedures or on developing integrated herd health management systems. This new initiative will permit early introduction of new animal health research findings into integrated livestock management systems.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

North Central Research Planning Committee
National Cattlemen's Association
American Dairy Science Association
National Turkey Federation
Animal Health Association
National Broiler Council

Animal Health and Disease

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Immunological Advances	20	$ 4,500	$ 4,000	$ 2,400	$ 720	$ 480	$12,100
Integrated Health Management	30	6,750	6,000	3,600	1,080	720	18,150
Animal Care	20	4,500	4,000	2,400	720	480	12,100
Embryo Transfer	30	6,750	6,000	3,600	1,080	720	18,150
SUBTOTALS		$22,500	$20,000	$12,000	$3,600	$2,400	
TOTALS	100	$42,500		$18,000			$60,500

GENETIC IMPROVEMENT OF ECONOMICALLY IMPORTANT PLANTS

RESEARCH INITIATIVE

To develop high quality plants that have the genetic capability of resisting pests and tolerating environmental stresses.

JUSTIFICATION

The cost of production and the productivity of many plants can be substantially altered through genetic improvement. By learning more about the sources of desired traits and the genetic basis of inheritance, teams of geneticists, biotechnologists, and plant breeders can work together to develop high quality, high-yielding varieties that are resistant to many common pests and environmental stresses. In addition, these genetic improvements can affect the uniformity of the product, the beginning dates and length of the growing season, plant health and vigor, and overall yield. Genetic improvements also would alleviate many of the current environmental concerns related to chemical and pesticide contamination.

OBJECTIVES

1. **Genetics and Breeding.** Genetic and breeding strategies must be developed that incorporate biotechnology and other innovative concepts with current breeding strategies to improve plant populations and enhance development of improved cultivars. Techniques are needed for identification and introduction of economically desirable traits and for improved breeding methodology to maximize the rate of genetic improvement. These findings can be used to develop highly productive genotypes and cultivars with improved yield and product quality.

2. **Pest Resistance.** Genetic bases must be found for minimizing the damage of such pests as nematodes, pathogens, insects, and weeds. Sources of resistance and the nature of resistance must be identified, and pest resistance must be incorporated into cultivars.

3. **Environmental Stress.** Genetic elements that determine responses to environmental stress factors must be identified. Strategies for incorporating these elements into improved cultivars are needed.

4. **Physiological and Morphological Parameters.** Genetic elements must be identified that determine superior physiological and morphological traits; strategies are needed for incorporating these traits into improved cultivars. Investigating the interactions among traits, environment, and cropping systems will allow scientists to identify superior genotypes.

5. **Soil Microorganisms.** The feasibility of increasing productivity and decreasing fertilizer and other inputs by enhancing plant capacity to utilize beneficial soil microorganisms must be determined. Plant nutrition will be enhanced by improving the associative relationships with rhizobia, mycorrhizae, and other organisms through selection of superior genotypes.

RELATIONSHIP TO BASE PROGRAM

The base program on genetic improvement of plants is broad and diverse. Much of the research is site specific due to geographic impact on plant performance. While the base program is broad, it lacks the depth, in many cases, to blend the products of ongoing research with the methods and products of new research in basic plant biology. The new initiative provides the interface between the "biotechnology initiative" and traditional breeding research; it amplifies the impact of both approaches.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

Crop Science Society of America
Southern Regional Research Committee
American Phytological Society
American Soybean Association
Western Agricultural Research Committee
National Grange
National Alliance of Industrial Crop Consultants
National Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences
ESCOP Symposium

Genetic Improvement of Economically Important Plants

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Genetics and Breeding	20	$2,000	$1,000	$1,600	$ 480	$ 320	$5,400
Pest Resistance	15	2,000	1,000	1,400	420	280	5,100
Environmental Stress	10	1,500	750	1,000	300	200	3,750
Physiological and Morphological Parameters	10	1,500	750	1,000	300	200	3,750
Soil Microorganisms	15	2,250	1,125	1,500	450	300	5,625
SUBTOTALS		$9,250	$4,625	$6,500	$1,950	$1,300	
TOTALS	70	$13,875			$9,750		$23,625

IMPROVED MANAGEMENT OF CROP PESTS AND DISEASES

RESEARCH INITIATIVE

To develop economically feasible and environmentally safe systems for managing diseases, insects, nematodes, and weeds.

JUSTIFICATION

Crop pests and diseases cause higher production costs and lower product quality. Improved pest management strategies are needed to cope with genetic modifications and population changes of pest and disease organisms. The previous initiative shows that through plant breeding and genetics we can reduce chemical requirements, deal with changes in pest populations, and improve product quality while preserving the environment. These scientific efforts need increased attention. Specific research is needed: 1) to improve pest population detection and identification using computer-based forecasting; 2) to study naturally occurring phenomena and their role in altering pest/pathogen-host interactions; and 3) to develop data processing programs for correlating crop and pest/disease data with climatic, edaphic, economic, and social factors.

OBJECTIVES

1. **Incidence, Prediction, Biocontrol.** Computer models that predict plant pest and disease occurrence and determine optimal control measures will be developed.

2. **Biocide Management.** Producers need more information about pesticide application methods, carriers, droplet size, weather effects, and formulations so that they can maximize the effects of pesticides on target organisms and minimize environmental contamination.

3. **Quantifying Constraints to Plant Productivity.** The ability of plants to tolerate biotic and abiotic stresses will be measured to determine which species have natural pest and disease resistances.

4. **Epidemiological Systems.** Rapid, economical and reliable methods for quantitatively analyzing pest and disease epidemics will be developed so that producers can forecast their occurrence, predict crop losses, and evaluate alternative management strategies.

5. **Biological Control Techniques.** New biological control agents and strategies for economically important plant pests and diseases will be developed.

6. **Integration of Pest and Disease Management into Crop Production Systems.** Producers need programs that integrate pest/disease management strategies into total crop production systems; these programs will be based on quantitative analysis of the biotic and abiotic factors affecting the relationship between pests/diseases and plants.

RELATIONSHIP TO BASE PROGRAM

Past pest and disease control programs have focused on a single pest or disease. More recently, research programs have focused on complexes of pests and diseases and the integration of pest and disease management into crop production systems. There are several key components to this process. Quantifying the biotic and abiotic factors that suppress the genetic potential of the plant is a relatively new approach and the current base program is modest. However, a large core of scientists is available to address the areas as the philosophical transitions take place. The base program in biological control techniques is substantial and growing; yet grossly inadequate relative to the opportunities and potential to increase agricultural productivity and decrease environmental contamination. Biocide management research needs enhancement as agricultural practices drastically change and new biocides, including those from biotechnology, are introduced into the agricultural system. Major advances are occurring in the use of computer models to predict pests and diseases and in the integration of pest and disease epidemiology and suppression tactics into crop production systems.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Association of Nurserymen
American Soybean Association
American Phytopathological Society
National Association of Independent Crop Consultants
North Central Regional Committee

Improved Management of Crop Pests and Diseases

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Incidence, Prediction, Biocontrol	20	$3,000	$1,500	$2,000	$ 600	$ 400	$7,500
Biocide Management	8	1,200	600	800	240	160	3,000
Quantifying Constraints to Plant Productivity	2	300	150	200	60	40	750
Epidemiological Systems	6	900	450	600	180	120	2,250
Biological Control Techniques	4	600	300	400	120	80	1,500
Integration of Pest and Disease Management into Crop Production Systems	10	1,500	750	1,000	300	200	3,750
SUBTOTALS		$7,500	$3,750	$5,000	$1,500	$1,000	
TOTALS	50	$11,250		$7,500			$18,750

AGRICULTURE IN THE URBAN ENVIRONMENT

RESEARCH INITIATIVE

To identify, select, and propagate plant materials and use them in creative environmental designs to enhance the quality of life.

JUSTIFICATION

The production of plants used in the urban environment is a new and very rapidly expanding area of agriculture in the United States. The economic impact of this new segment of production agriculture is greater than that of many other horticultural and row crop species. Through new research, much of the general knowledge developed for the production of food and fiber can be effectively transferred to the agriculture that supports the use of plants in the urban environment. These plant materials can be used to enhance the aesthetic value and alter the microenvironments of homes, workplaces, urban landscapes, and recreational areas. These modifications contribute to a sense of personal well-being among urban populations.

OBJECTIVES

1. **Plant Materials for Non-Traditional Environments.** Germplasm will be developed for the production of superior plant materials for urban environments.

2. **Maintenance Strategies for Plants in Diverse Environments.** Consumers of ornamental plants and turfgrasses need better maintenance strategies, new low maintenance plants with reduced fertility and water requirements, and pest management methods which reduce the use of pesticides in populous areas.

RELATIONSHIP TO BASE PROGRAM

Relatively little support exists in the base program for this area of research; yet, the new agricultural industries producing plants for the urban environment are multi-billion-dollar enterprises. This initiative will draw from general production strategies developed for food and fiber production, and apply that knowledge, along with new inputs from basic plant biology, to a new and highly profitable element of U.S. agriculture.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Association of Nurserymen
National Grange
Crop Science Society
Western Agricultural Research Committee

Agriculture in the Urban Environment

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Plant Materials for Non-Traditional Environments	30	$3,000	$1,500	$2,400	$ 720	$480	$8,100
Maintenance Strategies for Plants in Diverse Environments	20	2,000	1,000	1,600	480	320	5,400
SUBTOTALS		$5,000	$2,500	$4,000	$1,200	$800	
TOTALS	50	$7,500		$6,000			$13,500

SHORT-TERM ADJUSTMENTS FOR ENHANCING THE ECONOMICS OF AGRICULTURE

RESEARCH INITIATIVE

To develop short-term adjustment strategies for coping with adverse prices, costs, and other financial conditions affecting agriculture.

JUSTIFICATION

Increasing farm debts, declining product prices, rising production costs, and high real interest rates have forced many farmers into a struggle for economic survival. Farmers need radical, short-term adjustment strategies they can use to reduce unit production costs, production risks, and marketing uncertainty. Farming systems decisions and associated financial risk must be determined. Existing data from field production and marketing studies can be used to evaluate which adjustments might help ensure economic survival. Such short-term adjustments provide the basis for initiating longer-term changes as new production and marketing technologies become available.

OBJECTIVES

1. **Lower Unit Production Costs.** Farming systems will be developed which provide producers with ways of lowering production costs through crop rotations, cultural practices, and forage production.

2. **Improved Risk Management.** Producers need new ways to diversify their crop and livestock production and marketing enterprises in order to reduce the year-to-year variability in farm output, prices received, and net farm income.

3. **Alternative Financial Management Strategies.** Alternative financial management strategies must be developed to help producers cope with inflation, interest rates, foreign market uncertainties, and refinancing needs.

4. **Resource and Social Implications of Adjustments.** Researchers will provide strategies to ensure that the short-term adjustments proposed for improving farm survivability will not have adverse effects on water supply and quality, soil conservation, and the economic health of agribusinesses and rural communities.

RELATIONSHIP TO BASE PROGRAM

The base program of the SAESs is directed to developing production/marketing options for increasing yield and farm profitability. This increase will fund specific research to evaluate different production and marketing strategies in terms of high levels of inflation, interest rates, input prices, commodity prices and leverage. This data will help farmers and ranchers survive and prosper under turbulent conditions.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Farm Bureau
Grain Sorghum Producers Association
National Agricultural Chemicals Association
American Agricultural Economics Association
North Central Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
ESCOP Symposium

Short-Term Adjustments

Dollars x 1,000

Objective	Scientist Years	START-UP COSTS		ANNUAL COSTS			
		Facilities	Equipment	Personnel	Operating Expenses	Equipment	1st Yr. Total
Lower Unit Production Costs	20	$1,000	$2,000	$1,600	$ 480	$320	$5,400
Improved Risk Management	20	1,000	2,000	1,600	480	320	5,400
Alternative Financial Management Strategies	10	500	1,000	800	240	160	2,700
Resource and Social Implications of Adjustments	10	500	1,000	800	240	160	2,700
SUBTOTALS		$3,000	$6,000	$4,800	$1,440	$960	
TOTALS	60	$9,000		$7,200			$16,200

AGRICULTURAL POLICY IN A GLOBAL SETTING

RESEARCH INITIATIVE

To develop agricultural policies and institutions that will improve the performance and profitability of U.S. agriculture and strengthen its contribution to overall economic growth.

JUSTIFICATION

The health and strength of U.S. agriculture is affected by national and international economic policies. High real interest rates, flexible monetary exchange rates, and the well integrated international capital market have created a stressful situation for U.S. agriculture because of its increased dependence on foreign trade. Certain monetary and fiscal policies, as well as conditions in international financial markets, can cause farm commodity programs that operate by intervening in domestic markets to be counterproductive and costly to American farmers, consumers, and taxpayers. With this in mind, agricultural policy must be analyzed to take into account the interrelationships between commodity, factor, and financial markets at both the national and international levels. In this way, more realistic and beneficial policies and institutions can be developed.

OBJECTIVES

1. **Commodity, Factor, and Financial Market Relationships.** The relationships between commodity markets, factor markets, and financial markets at the national and international level will be analyzed.

2. **Political Economy of Domestic and Foreign Commodity Policy.** Research is needed to predict the policy reactions of each major agricultural trading country to U.S. commodity and trade policy.

3. **Comparative Productivity Growth and Competition in World Markets.** Factors that influence different rates of agricultural productivity growth among countries must be more fully defined, and their impacts on the absolute and comparative advantage in producing agricultural commodities for world markets must be determined in order to maximize returns to U.S. producers.

4. **Impacts of Technological Change.** The impact of agricultural technology on the well-being of agricultural producers, farm workers, rural communities, and the general population must be assessed.

5. **Policy and Institutional Design.** Policy reforms and institutional designs will be developed to improve the performance and profitability of U.S. agriculture within an interdependent world economy.

RELATIONSHIP TO BASE PROGRAM

The base program in agricultural policy has supported traditional commodity analysis and policy research within the State Agricultural Experiment Stations. Policy analysis is highly dependent on careful analysis of economic structure and knowledge of how decisions are made in the economy. The base program is inadequate to deal with the growing interdependence between the national and international economy and between the agricultural and non-agricultural sectors. The proposed increase will identify the specific provisions and likely consequences of the 1985 Agricultural and Food Policy Act; describe the structure of agriculture in different regions of the country; determine the income sources of farm families; identify any restrictions or lack of incentives to farm resource adjustments; investigate the linkages between agricultural and other markets and formalize these linkages; compare the alternative ways of measuring the effects of agricultural and macroeconomic policies; determine the causes and the policy implications of changing food consumption patterns; determine the impacts of food and agricultural policy on consumers' nutrition, health, and economic well-being; and analyze the effects of U.S. agricultural and trade policies on foreign supplies and trade. Lastly, the relationship between agricultural policy and the economy of rural communities will be established, especially in terms of services, employment, and economic activity.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Agricultural Economics Association
Southern Agricultural Research Committee
National Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences
ESCOP Symposium

Agricultural Policy in a Global Setting

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Commodity, Factor, and Financial Market Relationships	10	$ 500	$1,000	$ 800	$ 240	$160	$2,700
Political Economy of Domestic and Foreign Commodity Policy	10	500	1,000	800	240	160	2,700
Comparative Productivity Growth and Competition in World Markets	10	500	1,000	800	240	160	2,700
Impacts of Technological Change	10	500	1,000	800	240	160	2,700
Policy and Institutional Design	10	500	1,000	800	240	160	2,700
SUBTOTALS		$2,500	$5,000	$4,000	$1,200	$800	
TOTALS	50	$7,500		$6,000			$13,500

MARKET PENETRATION AND EFFICIENT MARKETING OF AGRICULTURAL AND FOREST PRODUCTS

RESEARCH INITIATIVE

To strengthen domestic and international markets for food, fiber, and forest products by increasing the variety, quality, availability, and utility of such products and by improving the structure and efficiency of markets.

JUSTIFICATION

Agricultural and forestry products have value only when provided in a form, time, and place that satisfies consumer demand. One major problem is the limited number of marketing outlets available to U. S. agriculture and forestry, even though both domestic and foreign consumers are increasingly able and willing to purchase more agricultural and forestry products. In order to satisfy both producer and consumer needs simultaneously, new knowledge is required regarding changing technologies, consumer tastes and preferences, and the scope of markets for food, fiber, and forest products. Such new information will guide future production, especially the variety, form, and quality of products. Also, improving the efficiency of processing and distribution operations will lower the costs of these products both at home and abroad and improve U.S. agriculture's market share in world trade.

OBJECTIVES

1. **Supply, Demand, and Price Relationships.** Basic information on domestic and foreign supply, demand, and price relationships for food, fiber, and forest products will be obtained.

2. **Grades and Standards.** Improving grades, standards, and other measures of product quality will better demonstrate the value of food, fiber, and forest products to the consumer.

3. **Market Efficiency and Performance.** Analyzing the efficiency and performance of alternative markets will help reduce costs, increase consumer satisfaction, and improve transportation systems.

4. **International Market Development.** Developing strategies for penetrating foreign markets will strengthen trade policy and reduce instability in international trade.

5. **Market Strategies and Power.** By analyzing the effects of various governmental marketing policies and regulations, and the market strategies of producer groups, methods can be developed to give more market power to agricultural producers, agribusinesses, and consumers.

RELATIONSHIP TO BASE PROGRAM

This initiative will bring new emphasis on international concerns to current market research. There has been an ongoing commitment to market research on isolated factors, and this initiatve should allow for a comprehensive approach to marketing. New tools and information sources are currently available and this initiative will ensure that our market research program addresses all of the components of marketing.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Farm Bureau
North Central Agricultural Research Committee
American Forage and Grasslands Council
National Cattlemen's Association
Northeastern Agricultural Research Committee
National Pork Producers Council
American Agricultural Economics Association
Southern Agricultural Research Committee
American Association of Nurserymen
Western Agricultural Research Committee
American Meat Science Association
National Agricultural Research Committee
ESCOP Symposium

Market Penetration and Efficient Marketing of Agricultural and Forest Products

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Supply, Demand, and Price Relationships	5	$ 250	$ 500	$ 400	$120	$ 80	$1,350
Grades and Standards	5	250	500	400	120	80	1,350
Market Efficiency and Performance	5	250	500	400	120	80	1,350
International Market Development	2	100	200	160	48	32	540
Market Strategies and Power	3	150	300	240	72	48	810
SUBTOTALS		$1,000	$2,000	$1,600	$480	$320	
TOTALS	20	$3,000		$2,400			$5,400

RURAL FAMILY AND COMMUNITY WELL-BEING

RESEARCH INITIATIVE

To enhance the economic, physical, social, and environmental well-being of rural families and communities.

JUSTIFICATION

The well-being of rural families, both those who are involved in agriculture and those who aren't, affects the quality of life in the communities in which they live. Their economic well-being depends upon their purchasing power, income stability, and resource management skills. Physical well-being is related to the availability of basic needs such as health care and housing, including housing that meets the special needs of the elderly and single-parent families. For social well-being, families must have access to community organizations and resources which provide support. Likewise, environmental well-being depends upon the use of proper safety practices both on and off the farm.

OBJECTIVES

1. **Family Stress Factors.** Analyzing family responses to stress resulting from social, economic, and technological changes will help families develop more effective coping strategies which contribute to individual, family, and community well-being.

2. **Environmental and Safety Factors for Families.** The safety of agricultural workers and their families can be improved by developing products and procedures which better protect them from pesticide exposure and other hazards.

3. **Economic Alternatives for Non-Metropolitan Areas.** The need for facilities and services (such as transportation, banking, education, and health care) for rural communities must be determined and alternative delivery systems developed. Strategies must be outlined for providing new employment opportunities and retraining programs to help rural residents adjust to social, economic, and technological changes.

4. **Organizing Capacities of Communities.** Strategies must be developed to enhance the vitality of rural communities through leadership development and through coordination of the community development efforts of public and private organizations and agencies.

5. **Future Governance Systems for Non-Metropolitan Areas.** The development of alternative organizational arrangements and funding mechanisms for governance areas within states will determine the future of counties, multi-county areas, municipalities and towns, and take into account the emergence of new governance units.

RELATIONSHIP TO BASE PROGRAM

Although the base program in the SAESs includes research related to families and communities, the research has not been designed to address the impact of changing conditions in agriculture of the nature and magnitude recently experienced by farm families and communities. Research focusing on family stress, changing social, economic and physical conditions, and family decision processes is needed. The current rural economic crisis underscores the need for research related to the creation of off-the-farm jobs and job retraining. Little has been done to study the effects of decentralization, deregulation, and levels of owner concentrations on rural communities.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Farm Bureau Federation
Farm and Industrial Equipment Institute
American Agricultural Economics Association
American Home Economics Association
Rural Sociological Society
Northeastern Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
ESCOP Home Economics Research Committee
ESCOP Planning Subcommittee
ESCOP Symposium

Rural Family and Community Well-Being

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Family Stress Factors	10	$1,000	$ 500	$ 800	$ 240	$160	$2,700
Environmental and Safety Factors for Families	5	500	250	400	120	80	1,350
Economic Alternatives for Non-Metropolitan Areas	5	500	250	400	120	80	1,350
Organizing Capacities of Communities	10	1,000	500	800	240	160	2,700
Future Governance Systems for Non-Metropolitan Areas	20	2,000	1,000	1,600	480	320	5,400
SUBTOTALS		$5,000	$2,500	$4,000	$1,200	$800	
TOTALS	50	$7,500		$6,000			$13,500

INTERRELATIONSHIPS OF FOOD AND THE NUTRITIONAL AND HEALTH STATUS OF PEOPLE

RESEARCH INITIATIVE

To improve the nutritional and health status of the population by determining the effects of food quality, bioavailability of nutrients, and dietary practices on health.

JUSTIFICATION

Food production and processing methods affect the safety and nutritional quality of food products. This, in turn, affects human health and nutrition. Research is needed on the social, cultural, ethnic, behavioral, economic, and environmental factors that influence consumer preferences and dietary practices. The bioavailability of nutrients from various sources and the interactions between foods and other substances must be evaluated. To accomplish these goals, researchers will need to develop improved methods of conducting human nutritional studies, which in turn will facilitate the design and assessment of nutrition intervention programs and policies.

OBJECTIVES

1. **Human Nutritional Requirements.** Researchers will determine more precisely the nutritional requirements of individuals as influenced by age, occupation, lifestyle, climatic conditions, and sex.

2. **Dietary Practices.** Identifying the economic, behavioral, social, cultural, and environmental factors that influence consumer preferences and dietary practices will improve nutrition education and lead to improved agricultural products to satisfy those needs.

3. **Nutritional Quality of Foodstuffs.** By studying the effects of production, processing, and preparation practices on the nutritional quality of food products, scientists can help improve the nutritional status of the population.

4. **Bioavailability of Nutrients.** Food nutrients interact with each other and with other substances individuals consume. Learning more about these interactions may lead to methods of improving the bioavailability of nutrients in food products.

5. **Dietary Health Risks.** More accurate information must be gathered about the possible health risks associated with certain food components such as lipids, saturated fats, cholesterol, chemical residues, and naturally occurring toxicants, so that consumers can make informed decisions.

RELATIONSHIP TO BASE PROGRAM

Aspects of current programs relate to nutrient requirements, dietary practices, nutrient composition, and bioavailability of nutrients. This initiative will not only make it possible to accelerate programs, but also to extend, expand, or strengthen needed research programs that have insufficient base funding. Given the high frequency with which new food processes and products are being introduced, this new effort will permit development of information regarding the effects of innovative processing techniques on the nutritional quality of food products, on the potential interaction(s) between or among nutrients or ingredients, and on the bioavailability of nutrients. Current research is exploring the effects of dietary practices and the possible health risks associated with specific food components. This initiative will make more precise information available to consumers and health professionals, so that they better understand the critical role of food constituents in the diet and their effects on each individual's health and well-being. This initiative also will facilitate the design and evaluation of nutrition intervention programs and public policies.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Farm Bureau Federation
American Forage and Grasslands Council
National Cattlemen's Association
American Dairy Science Association
American Home Economics Association
Institute of Food Technologists
North Central Agricultural Research Committee
Northeastern Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences
ESCOP Symposium

Interrelationships of Food and the Nutritional and Health Status of People

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Human Nutritional Requirements	10	$ 1,500	$ 750	$ 1,000	$ 300	$ 200	$ 3,750
Dietary Practices	10	1,000	500	800	240	160	2,700
Nutritional Quality of Foodstuffs	10	1,500	750	1,000	300	200	3,750
Bioavailability of Nutrients	20	3,000	1,500	2,000	600	400	7,500
Dietary Health Risks	50	11,250	10,000	6,000	1,800	1,200	30,250
SUBTOTALS		$18,250	$13,500	$10,800	$3,240	$2,160	
TOTALS	100	$31,750		$16,200			$47,950

BIOTECHNOLOGY

RESEARCH INITIATIVE

To understand the molecular and cellular processes in plants and animals and the inheritance of these functions, so that genetic materials can be manipulated to improve the production, quality, and quantity of food and fiber.

JUSTIFICATION

Because of recent developments in biotechnology, free-floating chains of DNA can be extracted from cells and the individual genes on these chains can be located, identified, and separated. The molecular arrangement of a specific gene can be modified and the gene reinserted into the chain, or a completely different gene with a more desirable trait can be selected from another cell and inserted. Even a "man-made" gene can be inserted. At the most fundamental level, biotechnology offers the opportunity to tailor crops and animals to the existing natural production environment. These modern methods can vastly accelerate traditional research processes and be used to generate new life forms that probably would not occur in nature.

OBJECTIVES

1. **Plant Productivity.** Recombinant DNA methods will be used to manipulate plant metabolism, alter chemical composition of plant products, improve processing quality, and produce plants resistant to stress or herbicides.

2. **Plant Disease Resistance.** Through genetic engineering, the development of plant resistance to diseases and insects will be accelerated.

3. **Nutritional Quality of Plants.** Genetic engineering will be used to correct nutrient deficiency, prevent loss of nutrients during storage, and eliminate or modify undesirable nutritional properties of plants, such as saturated fatty acid composition.

4. **Biological Control of Pests.** Highly specific organisms that adversely affect insect pests of crops and livestock will be produced.

5. **Biologically Active Materials.** Microorganisms and plant and animal tissue culture will be used to produce large quantities of biologically active materials such as growth and reproduction hormones and new vaccines or diagnostic tools.

6. **Diagnostic and Immunologic Products.** New vaccines and diagnostics for animal agriculture will be produced.

7. **Animal Disease Resistance.** The impact of animal diseases can be reduced by developing genetic disease resistance in livestock species.

8. **Animal Development and Productivity.** Natural breeding programs will be augmented and improved by incorporating genetic engineering methods to enhance productivity in livestock, and to reduce levels of saturated fat in meats while retaining palatability.

9. **Environmental Impact of Biotechnology.** The potential impact of introducing genetically engineered organisms into the environment will be assessed, as well as the effect of biotechnology on the size of agricultural operations and the structure of rural communities.

RELATIONSHIP TO BASE PROGRAM

This initiative will hasten the development of new tools, new techniques, and new concepts for agricultural advancements. Relative to the base program, biotechnology research in agriculture represents an entirely new and unique thrust. In recent years, the U.S. agricultural research system has made major commitments to developing biotechnology for our industry. Our rate of progress has been limited by insufficient funding for personnel, instrumentation, and facilities. The competitiveness of American agriculture is at stake as the international community hastens to exploit the emerging opportunities. Additional resources must come to the system if we are to meet these challenges.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

North Central Regional Research Planning Committee
Northeast Regional Research Committee
Southern Regional Research Committee
Western Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences
American Dairy Science Association
American Meat Science Association
American Phytopathological Society
American Society of Animal Science
Soil Science Society
Crop Science Society
American Forage & Grass Council
American Soybean Association
Grain Sorghum Producers
National Pork Producers Council
American Association of Nurserymen
National Alliance of Independent Crop Consultants

Biotechnology

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Plant Productivity	30	$ 6,750	$ 6,000	$ 3,600	$1,080	$ 720	$18,150
Plant Disease Resistance	20	4,500	4,000	2,400	720	480	12,100
Nutritional Quality of Plants	20	4,500	4,000	2,400	720	480	12,100
Biological Control of Pests	20	4,500	4,000	2,400	720	480	12,100
Biologically Active Materials	20	6,750	6,000	3,600	1,080	720	18,150
Diagnostic and Immunologic Products	30	6,750	6,000	3,600	1,080	720	18,150
Animal Disease Resistance	20	4,500	4,000	2,400	720	480	12,100
Animal Development and Productivity	30	6,750	6,000	3,600	1,080	720	18,150
Environmental Impact of Biotechnology	15	3,375	3,000	1,800	540	360	9,075
SUBTOTALS		$48,375	$43,000	$25,800	$7,740	$5,160	
TOTALS	205	$91,375		$38,700			$130,075

COMPUTER TECHNOLOGY FOR AGRICULTURAL MANAGEMENT

RESEARCH INITIATIVE

To develop user-oriented computer models for electronic communication, data management, and production and marketing decisions.

JUSTIFICATION

Computer technology can bring a higher level of management capability to agricultural production and marketing. Expert systems and multi-resource, multi-enterprise decision models are needed to help producers reduce costs, increase profits, and use resources more efficiently. The complexity of crop production, crop rotation, pest management, irrigation, fertilization, chemical management, quality control, and marketing can be overcome with decision-making software packages for producers.

OBJECTIVES

1. **Production Management/Decision Models.** Quantitative expressions of production components will be developed so that they can be integrated into computerized management/decision models. Initially, such models will be crop and location specific.

2. **Farm-Related Cost Accounting Models.** The development of accurate cost accounting models for each component of the farm enterprise will enable producers to make better, more timely management decisions.

3. **Expert Systems for Management/Decision Making.** Integrated farming systems models which incorporate all the variables in production and marketing must be developed. Such models, using expert systems, will contribute to the efficient use of natural resources and the economic competitiveness of agriculture.

RELATIONSHIP TO BASE PROGRAM

Much present research in agriculture and forestry focuses on the relationships between production variables, improving and protecting plants and animals, and efficient resource use. This new initiative will permit more effective integration of the knowledge gained through these research activities through the use of computer technology. More intensive effort will be devoted to the development of integrated, computerized, management/decision models for use by individual producers. Similarly, more intensive effort will be devoted to developing "user-friendly" versions of accounting models for the individual components of the farm enterprise. Intensifying these efforts will facilitate the most unique aspect of this initiative—the development of expert systems to assist producers in managing agricultural and natural resources. Such expert systems will combine the intuitive and fragmented scientific knowledge currently available with the knowledge developed in the other thrusts under this initiative to more effectively serve the individual producer.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

National Agricultural Chemicals Association
National Agricultural Research Committee
ESCOP Council
ESCOP Special Initiatives Subcommittee
USDA Joint Council on Food and Agricultural Sciences

Computer Technology for Agricultural Management

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Production Management/ Decision Models	20	$3,300	$ 5,200	$2,400	$ 720	$ 480	$12,100
Farm-Related Cost Accounting Models	10	1,650	2,600	1,200	360	240	6,050
Expert Systems for Management/Decision Making	20	3,300	5,200	2,400	720	480	12,100
SUBTOTALS		$8,250	$13,000	$6,000	$1,800	$1,200	
TOTALS	50	$21,250		$9,000			$30,250

ROBOTICS IN AGRICULTURE

RESEARCH INITIATIVE

To integrate electronic sensors, computer hardware and software, and mechanical equipment into electronic control systems that aid in the management and control of agricultural production and processing operations.

JUSTIFICATION

The control of agricultural production and processing is usually done by human operators or by simple automatic control systems which measure a single variable such as temperature, humidity, or time. In many cases, the operator assumes that process variables are constant over a period of time, although they actually change frequently. Sensors are not currently available for effectively measuring important variables such as soil moisture, plant growth, health of animals, animal performance, moisture content of flowing grain, and rheological properties of food products. Adequate control and management of a production or processing unit depends on sensing all pertinent variables simultaneously and using the results to make an appropriate decision. Modern sensory systems include sensors and decision-making algorithms in on-line computers, and serve either as automatic controls for activating appropriate devices or as tactical management aids. More effective sensors are important for realizing the potential benefits of computers in agriculture. Improved electronic systems can help operators take more precise actions, resulting in greater output, more efficient use of energy and material resources, a safer workplace for human operators, and increased economic competitiveness.

OBJECTIVES

1. **Research for Sensor Technology Development.** The quantitative relationships between biophysical properties and measurable properties must be determined in order to develop sensors which measure the state of biological materials and then integrate them into electronic systems for controlling agricultural operations.

2. **Electronic Systems for Plant Production.** Producers of field crops and forestry and nursery products need robotic systems which can measure physical and chemical soil fertility, control automatic tillage and chemical application operations, control direct seed placement, handle transplants and plants regenerated in tissue culture, and monitor micro- and macro-environments of plants. Improved sensory systems will lead to more efficient use of resources and improve the economic competitiveness of producers.

3. **Electronic Systems for Animal Production.** By developing sensors that continuously monitor animal environments, animal health and stress, animal performance, and animal feed, automatic control systems can be developed to alter the environment and feed mix to improve animal health and performance, optimize energy use, and improve economic competitiveness.

4. **Electronic Systems for Food Processing.** Sensors that measure product quality and the variables involved in food processing will be developed in order to improve the efficiency of resource and labor use, enhance food quality, and improve the economic competitiveness of U.S. agriculture.

RELATIONSHIP TO BASE PROGRAMS

Current research supported by base program funding is primarily directed toward meeting the needs of research instrumentation. This new initiative will permit an increase in all research objectives to improve agricultural profitability by reducing production costs while improving products and developing new uses for farm products. These objectives are, to a major degree, dependent upon production systems which are adaptive and flexible. The new initiative will enable researchers to develop sensors and computer software which integrates multiple-source sensor information to provide machine intelligence. With these innovations, industry will be able to produce the systems needed by agriculture.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Association of Nurserymen
Farm and Industrial Equipment Institute
National Grange
American Agricultural Economics Association
American Society of Agricultural Engineers
Fish and Allied Aquatics Society
Institute of Food Technologists
Soil Science Society of America
Society of Wood Science and Technology
ESCOP Special Initiatives Subcommittee
North Central Agricultural Research Committee
Southern Agricultural Research Committee
Western Agricultural Research Committee

Robotics in Agriculture

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Research for Sensor Technology Development	10	$2,250	$2,000	$1,200	$360	$240	$6,050
Electronic Systems for Plant Production	5	1,125	1,000	600	180	120	3,025
Electronic Systems for Animal Production	5	1,125	1,000	600	180	120	3,025
Electronic Systems for Food Processing	5	1,125	1,000	600	180	120	3,025
SUBTOTALS		$5,625	$5,000	$3,000	$900	$600	
TOTALS	25	$10,625		$4,500			$15,125

PROCESSING AND QUALITY ENHANCEMENT

RESEARCH INITIATIVE

To incorporate recent advances in biotechnology, electronic sensors, and robotic controls to improve food processing systems and ensure safe and nutritious food products. To understand the physical, chemical, and biological properties and structures of foods and related products, as well as the mechanisms that control biological activity.

JUSTIFICATION

On average, more than 60 cents of every retail food dollar pays for post-harvest activity, which includes processing, transportation, storage, preservation, packaging, and marketing. Consumers are demanding improvements in the safety, wholesomeness, and nutritional makeup of their foodstuffs. In addition, growing environmental concerns are forcing additional expense to be incurred in handling, recycling, and disposing of agricultural wastes and residues. Emerging technologies offer opportunities to substantially improve the quality and reduce the cost of food products by improving post-harvest activities.

OBJECTIVES

1. **Processing.** This research will characterize the molecular and structural properties of foods and their effects, employ new electronic sensing and robotic technologies to improve existing processing methods and develop new food processing systems, and match food composition and availability with human nutrient needs.

2. **Quality Enhancement.** The structural/functional relationships of food materials will be studied to improve the quality of food and the utilization of nutrients. Enzymes will be isolated, characterized, and used in food processing. Recent technological advances will be applied to the control of microorganisms involved in food production.

3. **Food Safety.** This research will identify the factors that affect food selection, the role of food components in health, and human nutrient requirements. Methods of rapidly determining food components will be developed. Preservation techniques will be improved by identifying foodborne diseases and microbial and other toxins. Improved methods of toxicity testing will be developed.

4. **By-Products and the Environment.** The composition and properties of process waters and waste will be determined in order to devise better methods of disposal and assess their potential for use in value-added by-products. New sanitizing/cleaning agents and methods will be developed. The value of food process residues as fuels or as soil amenders will be determined.

RELATIONSHIP TO BASE PROGRAM

This new initiative will make it possible to generate and apply new information regarding the molecular, structural, and functional properties of foods to the improvement of existing processing methods and the development of new food processing systems. New systems may use electronic and robotic technologies. Due to restricted base funding, the modification of food enzymes and microorganisms through biotechnology is just beginning to be explored. New initiatives will take advantage of breakthroughs in this area. Current research seeks to improve processes by controlling safety hazards, and to identify, characterize, and eliminate foodborne disease and microbial and other toxins. New efforts will provide information regarding toxic effects on humans, as well as improved methods for toxicity testing. A new thrust, aimed at the recovery of useful, value-added products from processing by-products and process waste streams, will allow adaptation of new membrane and other separation technology to food processing systems.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

ESCOP Symposium
National Agricultural Research Committee
Southern Agricultural Research Committee
Western Regional Agricultural Research Committee
Institute of Food Technology
American Meat Science Association
Crop Science Society
American Phytopathological Society
National Cattlemen's Association
Grain Sorghum Producers Association
National Grange

Processing and Quality Enhancement

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Processing	30	$ 6,750	$ 6,000	$ 3,600	$1,080	$ 720	$18,150
Quality Enhancement	20	4,500	4,000	2,400	720	480	12,100
Food Safety	30	6,750	6,000	3,600	1,080	720	18,150
By-Products and the Environment	20	4,500	4,000	2,400	720	480	12,100
SUBTOTALS		$22,500	$20,000	$12,000	$3,600	$2,400	
TOTALS	100	$42,500		$18,000			$60,500

ENERGY EFFICIENT SYSTEMS

RESEARCH INITIATIVE

To develop energy-efficient production and processing systems for agriculture and forestry.

JUSTIFICATION

Energy costs are a major component of the total costs of producing and processing agricultural and forestry products. Energy inputs for fertilizer, pesticides, crop drying, heating and processing are likely to become even more costly in the future. Food production, processing, and distribution consumes 17 percent of the total energy used in the United States. Because the needs for energy in agriculture are seasonal, short-term disruptions in supply can have serious effects. In order to cut energy costs and ensure a stable, continuous energy supply for agricultural and forestry enterprises, producers need more energy-efficient production and processing systems, more efficient power units, and alternative fuels.

OBJECTIVES

1. **Efficient Plant and Animal Production and Processing Systems.** Developing production systems that require less energy and fuel will increase the profitability of agriculture, forestry, and agribusiness.

2. **Conversion Technology.** Producers need conversion technologies that will permit efficient liquid and gaseous fuel production from biomass crops. These fuels can be used on the farm, especially for crop drying, as well as off the farm for space heating.

3. **Energy Assessment.** Assessing the energy efficiency of alternative agricultural and forestry production and processing systems will help determine ways of reducing costs.

RELATIONSHIP TO BASE PROGRAM

The current base program assesses the energy efficiency of food and fiber production systems, but increased funding will be required to make substantial progress in conversion technology and in developing more efficient systems. The human and physical resource base is in place as a result of prior external funding, and could effectively utilize additional funds to make possible the commercialization of conversion technology and the improved efficiency of production systems.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

Farm and Industrial Equipment Institute
Crop Science Society of America
National Grange
Western Agricultural Research Committee
American Forage and Grassland Council
National Agricultural Research Committee
American Society of Agricultural Engineers
ESCOP Symposium

Energy Efficient Systems

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Efficient Plant and Animal Production and Processing Systems	10	$1,500	$ 750	$1,000	$300	$200	$3,750
Conversion Technology	6	1,350	1,200	720	216	144	3,630
Energy Assessment	4	400	200	320	96	64	1,080
SUBTOTALS		$3,250	$2,150	$2,040	$612	$408	
TOTALS	20	$5,400		$3,060			$8,460

INTEGRATING AGRICULTURAL TECHNOLOGY

RESEARCH INITIATIVE

To develop production and marketing systems that will sustain the long-term productivity and profitability of agricultural operations.

JUSTIFICATION

The long-term profitability of farming is affected by technological changes, quantity and quality of farm resources, prices of farm inputs and products, policies of financial institutions, and government farm programs. Further research is needed on how to integrate production technology into whole-farm management. Producers need new farming systems that encompass biotechnology, computer technology, marketing alternatives, and institutional innovations for financing farming operations. More complex organizational structures that allow for a combination of farm and off-farm income, involve vertical and/or horizontal integration in production and marketing, and depart from the traditional farm firm need to be developed and analyzed with respect to their long-term profitability. Such information would enable producers to develop production, marketing, and investment strategies for making long-term adjustments in their farming operations.

OBJECTIVES

1. **Assessment of New Production Technologies.** Analyses of the economic feasibility of emerging biotechnologies and information technologies, and their impacts on risk management, will help producers make decisions about adoption of new technologies.

2. **Impacts of Market Forces on Enterprise Profitability.** To increase the long-run profitability of their production enterprises, farmers need to have analyses of the effects of various market factors on production costs, commodity prices, and investment capital costs.

3. **Capital Investment and Financial Requirements.** Analyzing the investment capital costs of adopting new technologies will help producers develop financial strategies for meeting these investment needs.

4. **Integrated Farming Systems.** To help farmers survive in times of economic uncertainty, profitable farming systems must be developed that integrate financial arrangements, marketing options, production technologies, resource management practices, and enterprise alternatives for various crops and/or livestock operations.

RELATIONSHIP TO BASE PROGRAM

The base program has tended to focus on individual commodities. The next step in farming systems research is to integrate various combinations of crop growth and/or animal production techniques with marketing and financial options. As the emerging biotechnologies and information technologies become available, they must be assessed within the context of current production knowledge to determine their long-term profitability. Funding proposed for this research initiative will provide for improved farm management decisions through 1990. The integration of traditional data with information available through new disciplines such as knowledge engineering and biotechnology will allow for timely and accurate production, financial, and marketing strategies, while maximizing the opportunities for profitability.

ORGANIZATIONS IDENTIFYING RESEARCH NEEDS

American Farm Bureau Federation
American Forage and Grasslands Council
American Soybean Association
National Agricultural Chemicals Association
National Grange
National Pork Producers Council
American Agricultural Economics Association
Crop Science Society of America
National Alliance of Independent Crop Consultants
North Central Agricultural Research Committee
Northeastern Agricultural Research Committee
Southern Agricultural Research Committee
Western Agricultural Research Committee
National Agricultural Research Committee
USDA Joint Council on Food and Agricultural Sciences
ESCOP Symposium

Integrating Agricultural Technology

Dollars x 1,000

		START-UP COSTS		ANNUAL COSTS			
Objective	**Scientist Years**	**Facilities**	**Equipment**	**Personnel**	**Operating Expenses**	**Equipment**	**1st Yr. Total**
Assessment of New Production Technologies	20	$ 2,000	$1,000	$1,600	$ 480	$ 320	$5,400
Impact of Market Forces on Enterprise Profitability	30	3,000	1,500	2,400	720	480	8,100
Capital Investment and Financial Requirements	20	2,000	1,000	1,600	480	320	5,400
Integrated Farming Systems	30	3,000	1,500	2,400	720	480	8,100
SUBTOTALS		$10,000	$5,000	$8,000	$2,400	$1,600	
TOTALS	100	$15,000		$12,000			$27,000

APPENDIX 4—ADDITIONAL MATERIALS SUBMITTED FOR THE RECORD BY DR. WILLIAM F. BRINKMAN

PHYSICS THROUGH THE 1990s

A Summary

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NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors, according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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A Summary

PHYSICS THROUGH THE 1990s

A Summary

This summary presents some highlights of the recently released physics survey *Physics Through the 1990s*. The purpose of the survey is to provide an authoritative research assessment of the major fields of physics. To accomplish that, the Physics Survey Committee prepared six volumes on the fields of physics and two cross-cutting volumes—*Scientific Interfaces and Technological Applications* and *An Overview*. This brief summary can do little more than pique the reader's interest in the more complete treatment in those eight volumes, which provide convincing documentation of a physics enterprise that is vital and productive.

The summary is organized into three parts. Section 1 sketches the progress that has occurred over the past decade and gives an indication of where the field is headed. Only a few highlights have been included; in each field of physics, important elements have been omitted for the sake of brevity. Section 2 relates physics to other sciences and the needs of society. The first two sections are quite selective; applications have received strong emphasis as a principal theme of this summary. Section 3 indicates some of the requirements for maintaining excellence in physics and includes a brief synopsis of the major recommendations of the Physics Survey Committee.

1. PROGRESS IN PHYSICS—SOME HIGHLIGHTS

Elementary-Particle Physics

Elementary-particle physics is the study of the basic nature of matter and force. Elementary-particle physicists seek the fundamental constituents of matter and the forces that govern their behavior.

Over the past hundred years, physicists found that matter could be broken down into molecules and atoms, that an atom consists of a nucleus surrounded by electrons, and that a nucleus is composed of protons and neutrons. Except for the electron, none of these pieces of matter was found to be simple or elementary.

Then in the past two decades, tremendous progress was made using a new generation of particle accelerators and radically new theoretical ideas. Matter has been found to consist of just two types of elementary particles—quarks, of which protons and neutrons are made, and leptons, such as the electron. And all the complicated forces that act on matter have been shown to reduce to just three fundamental kinds: the gravitational force, the unified electromagnetic-weak force, and the strong nuclear force.

This new level of understanding of matter and forces has allowed physicists to raise new deeper questions that could not have been asked before. We still do not understand what determines the basic properties of quarks and leptons such as their weight and stability. Furthermore, the mathematical resemblance among the fundamental forces suggests the possibility of a profound unification: perhaps all forces are different manifestations of the same basic force; perhaps all nature reduces to some deep symmetry. Can we find the basic force? Can we find that deep symmetry?

To answer these questions and to carry out experiments in the unknown territory beyond these questions requires new, more powerful particle accelerators using new technologies and new theoretical ideas. The size and cost of the most powerful particle accelerators require that they be national enterprises and that full use be made of international cooperation where practical.

Nuclear Physics

Nuclear physicists are concerned with the many facets of atomic nuclei—their sizes, their composition, and under what conditions they come apart. We now know that protons and neutrons, once thought to be the ultimate constituents of nuclei, are made out of quarks. No one has ever observed an isolated quark. One of the basic challenges facing nuclear physics today is the detailed exploration of the behavior of quarks inside nuclei in nuclear collisions induced with the help of powerful particle accelerators. Calculations predict that a quark-gluon plasma will be produced—a new state of matter with extremely high density similar to that which may have existed in the earliest moments of the universe and may exist now during supernova explosions. This exciting frontier may lead to a more basic understanding not only of nuclei but also of the evolution of our universe.

In an effort to study the properties of nuclei and the forces holding them together, nuclear physicists are bombarding nuclei with powerful projectiles such as electrons, protons, and nuclei themselves. Much can be learned about the way nuclei are excited and also about their electric and magnetic properties, quantities that are important in the application of physics to fields like medicine and geology, as discussed below. Nuclear physics also continues to be a source of information required for the deeper understanding of the fundamental laws of nature.

Atomic, Molecular, and Optical Physics

Atomic, molecular, and optical physics share such similar points of view and so many common techniques that the three join naturally into a single field. They also share a goal: to understand the structure of atoms and simple molecules and how they interact with each other and with light. The field also provides techniques and devices for many other areas of science and technology. For example, it provides instruments for environmental engineering and for superaccurate metrology.

One new device is a particle trap, which consists of an electromagnetic field shaped so that it traps charges—even single electrons or ions—and holds them for months at a time. Such traps allow extraordinarily precise measurements to be made. Experiments with single electrons have provided one of the most precise tests of modern quantum theory. Particle traps also provide new ways to study collisions and reactions between molecules and promise a new generation of ultraprecise atomic clocks.

Atomic clocks keep time by using one of the internal motions of an atom as a pendulum. Because all atoms of a given species radiate at identical frequencies, atomic clocks can be fantastically precise. Atomic clocks that are accurate to one second in a hundred thousand years are widely employed; for specialized uses, clocks have reached a precision of one second in ten million years. Atomic clocks are essential to global-scale radio telescopes that need to synchronize signals exactly and for global positioning systems that allow navigators to compute their positions to within 10 meters.

The most dramatic device both created and used by atomic, molecular, and optical physics is the laser. Lasers cause atoms and molecules to radiate their light in unison and in a single direction, producing a light a million times more pure in color than previously possible. A laser on the Earth with a power of a few watts is visible on the moon. Lasers have revolutionized atomic, molecular, and optical physics and have found applications in almost every branch of science and in medicine and industry. Their uses include atmospheric analysis, laser surgery, fiber-optic communication, laser printing, home electronics, laser-assisted manufacturing, the creation and study of new atomic and molecular species, tests of relativity theory, studies of polymers and liquid crystals, and measurements of biological materials.

In the 1970s physicists learned how to use lasers to generate flashes of light only 10^{-12} sec in duration. Now lasers can generate light pulses a thousand times shorter, lasting a few femtoseconds (10^{-15} sec). Femtosecond pulses are so quick that the light barely has time to travel a distance of one hundredth of the thickness of a human hair. Femtosecond lasers can be used to take snapshots or even movies of molecules during a chemical reaction or of electrons as they carry heat in a metal. Such movies make it possible to design more efficient combustion engines and to control combustion by-products. In the future, new types of high-speed computers may be able to transmit signals by short pulses of light rather than by the relatively sluggish electrons now used.

Plasmas and Fluids

Understanding how fluids flow is essential to understanding phenomena as diverse as combustion, blood flow, sound waves, aerodynamics, and plate tectonics. Insight into plasmas implies understanding most of the matter in the universe: the Sun, stars, magnetospheres, stellar winds, and galaxies are all formed of plasmas. Modeling such complex nonlinear systems entails using sophisticated diagnostic techniques and high-speed computers.

A plasma is a gas so hot that many of its atoms have become disassociated, leaving electrons (with a negative charge) and partially stripped atoms (with a net positive charge). Much research into plasmas has been for an earthly reason: they are the fuel for nuclear fusion, which may one day provide a plentiful supply of energy. In theory plasmas can be confined and heated to the point where their nuclei fuse together, giving off enormous amounts of energy. In practice, fusion is difficult to achieve and control. Nevertheless, significant technical progress has been made, and laboratory fusion plasmas are beginning to enter the range of breakeven-equivalent conditions in deuterium plasmas. That is, the fusion power that would be produced with a deuterium-tritium fuel mixture will be approximately equal to the power required to maintain the plasma temperature.

Recently, physicists have discovered that plasmas can be used to boost the energy of charged particles. This phenomenon holds out the promise of a smaller alternative to conventional particle accelerators. By shooting laser beams into a plasma, plasma waves that are accompanied by huge electric fields traveling at nearly the speed of light are created. These fields can, in principle, be used to speed up particles. An accelerator using this principle (called a beat-wave accelerator) would generate electric fields a thousand times larger than those found in conventional particle accelerators. A beat-wave accelerator could be substantially smaller than a conventional one but could produce particles with similar energies. Development of a practical particle accelerator based on this principle may lie many years in the future.

Research on fusion plasmas and beat-wave accelerators is leading to rapid advances in understanding the plasma state. Plasmas and fluids are extremely complicated systems. Technical advances depend on a fundamental understanding of nonlinear effects and turbulence. Recent breakthroughs in theory, mathematics, and computer speed have led to what physicists call a theory of chaos. Smoke entrained in the hot air leaving the tip of a cigarette rises smoothly at first. Then it swirls into small eddies, which become larger and then break up into a new generation of smaller eddies. The breakup of eddies is one example of chaotic motion; others are population biology, the patterns of the orbits of asteroids, and the circulation of the atmosphere. Physicists are using high-speed computers to model how chaotic motion starts and how it develops.

Condensed-Matter Physics

Matter is found in four states—gas, plasma, liquid, and solid. Condensed-matter physicists study the properties of the latter two states.

In the past, condensed-matter physicists idealized solid matter as an infinite, orderly array of atoms. In their models, the solid was entirely bulk; its surfaces could be neglected. In the past decade, condensed-matter physicists have turned their attention to more realistic and more complex models—materials that have little or no order, artificially structured materials with new kinds of order, and surfaces whose properties differ from the properties of the bulk.

Condensed-matter physicists use various techniques to create solids not found in nature. One technique is to cool a liquid so rapidly that its atoms do not have time to take up their normal crystalline order. The resulting solid remains amorphous. One consequence is that, with no crystal planes to slip past each other, the material can be much stronger.

Another technique is to deposit a layer only atoms thick on a clean surface. The layer is, for all practical purposes, a two-dimensional material. Such materials have unusual optical and electronic properties and are used, for instance, to make light-emitting diodes.

A third technique is to deposit alternating two-dimensional layers of different substances, laying one on top of another. Such a material is called a superlattice. By changing the materials and thicknesses of the layers, physicists can tailor the superlattice to have special properties: to conduct electricity slowly or quickly, to change conductivity with temperature, or to have controllable magnetic properties.

The new materials and new methods of fabricating them are not only of intrinsic scientific interest, but they also have important technological applications. The development of various technologies depends not only on being able to tailor the strength and electronic properties of materials but also on knowledge of what goes on at the surfaces of materials and at the interfaces between materials. Computer chips, for instance, have become so small that surfaces play a dominant role. And chemical reactions vital to industry, especially catalysis and oxidation, take place at surfaces. Of all branches of physics, condensed-matter physics most directly fuels advances in technology. Many of the applications discussed in Section 2 of this summary derive from condensed-matter physics.

Gravitational, Cosmological, and Cosmic-Ray Physics

We have new eyes with which to look at the universe, and they are changing our notions of it. In addition to powerful ground-based telescopes for optical and radio wavelengths, telescopes in orbit now detect infrared, ultraviolet, x-ray, and gamma-ray wavelengths that come to us from stars, galaxies, and the edges of the universe. Cosmic rays bring with them information about such energetic processes as the explosions of supernovae.

Gravitational physicists hope that gravity waves will open another window on the universe. Einstein theorized that, like moving charged particles that emit electromagnetic waves, moving masses emit waves of gravity. These waves are extremely weak. Only when the masses involved are of the size of the Sun and planets is there any significant generation of gravitational wave energy. For example, the gravitational wave glow of the Earth as it orbits the Sun is only about 200 watts—as much as a typical room lamp. However, when a massive star collapses, it can radiate an enormous flash of gravitational waves with an energy equivalent to a solar mass. Recently, experimental bar detectors in the laboratory, cooled to temperatures very close to absolute zero and suspended in almost complete isolation from ambient acoustic noise, have achieved large gains in sensitivity that approach the range predicted for astrophysical catastrophes such as the formation and coalescence of black holes. By the turn of the century, interferometric detectors of very large dimensions and far greater sensitivity may be flown in space.

Cosmology's field is the origin and evolution of the universe. Fifty years ago, cosmologists theorized that the universe began in a gigantic explosion; thirty years later, physicists detected the afterglow of that explosion. Now the big-bang theory is widely accepted. In the past decade, cosmologists have been trying to piece together a consistent picture of how the universe behaves on the largest scales.

The problems are challenging. Will the universe continue to expand forever? Or will it slow down, reverse, and finally collapse? Do the stars and galaxies that we see constitute the dominant matter in the universe? Or is most of the matter invisible, made perhaps of particles that exist in theory but are not yet detected? How and when were stars and galaxies first formed in the universe? What accounts for the huge voids and the complex filaments of galaxies?

In the past decade, cosmologists have gone to elementary-particle physics for answers to questions about the early universe. For instance, particle physicists' Grand Unification Theories might explain the puzzling ratio of matter to radiation in the universe. Particle physicists have also postulated several new particles that might seed galaxy formation or determine how the universe expands. Since the same particles might also make up the invisible matter, cosmologists are now designing experiments to find them. Such a discovery would be a milestone in physics.

2. SCIENTIFIC INTERFACES AND TECHNOLOGICAL APPLICATIONS—PHYSICS, OTHER SCIENCES, AND SOCIETY

Physics is fundamental to all natural sciences. The particles and atoms that physics studies compose proteins, genes, organs, living bodies, all man-made and natural materials, the earth, the oceans, and the atmosphere. In this sense, physics underlies chemistry, biology, materials science, and geophysics. Basic concepts and techniques from physics are used throughout all the physical sciences. New scientific disciplines have arisen at the interfaces between these sciences and physics, creating a scientific whole that far exceeds the sum of its parts. Interdisciplinary science is one of the brightest promises of the coming decade.

Another of the next decade's bright promises is technology: we are in the midst of a second industrial revolution. Lasers, transistors, semiconductors, magnetic tape, and computer chips have drastically changed our society. These and other technologies depend on basic research in physics. For example, the principles of magnetic resonance imaging, a revolutionary noninvasive technique for seeing inside the body, were discovered by physicists Edward Purcell and Felix Bloch, who were curious about how nuclei and matter interact magnetically. By studying the way current flows through a semiconductor under different conditions, William Shockley, John Bardeen, and Walter Brattain discovered the transistor effect and opened the way for integrated circuits, microelectronics, and the whole computer revolution. Arthur Schawlow and Charles Townes were studying the effects of light on molecules and solids; what they found led to the laser.

The time between a new idea in physics and its application in technology is becoming shorter and shorter. High-speed electronics, optical communication, advanced medical instrumentation, and exotic defense systems have all grown to maturity within a few years of the discovery of their basic principles. The futures of both technology and the natural sciences depend profoundly on new interfaces with, and applications of, physics.

Geophysics

Geophysics, born from the merger of geology and physics, studies the structure and dynamics of the solid earth, the oceans, and the atmosphere. Physics provides not only many of the fundamental models of geophysics but its instruments as well.

The circulation of the oceans, the interactions between the oceans and the atmosphere, the patterns of the weather, and models of the greenhouse effect are all in part problems in two- and three-dimensional turbulence. Turbulence remains one of the major unsolved problems of physics. Nevertheless, geophysicists are now able to put their observations and the requisite physics into computer simulations and generate realistic models of the oceans and atmosphere. Some of the largest and fastest computers have been built specifically for research into these problems. Their solutions, however, await computers that are still larger by orders of magnitude.

Twenty years ago, the positions of mountain chains, earthquakes, and volcanoes were well charted but not understood. Geophysicists now know that the surface of the Earth is broken into several rigid plates, moving with respect to each other at rates of 1 to 10 centimeters every year. The plates float on a thick layer of denser material called the mantle; they move because the mantle does. The mantle rises in some places, creating ridges that run down the center of the oceans, and sinks in others, pulling the edges of continents back into the mantle. Earthquakes occur where the plates rub past each other, mountains build where plates collide or where one dives under the edge of another, and volcanoes erupt wherever the mantle rises close to the surface. Physics explains the forces governing these movements; physics also supplies the instruments that chart the movements. Seismographs, by mapping out the distribution of earthquakes, have also delineated the plates. Seismographs and sound-wave detectors set out in arrays can be coordinated to give tomographic profiles of the layers of the oceans, the entire ocean floor (now we can see barely a fraction of the ocean floor, and that only sporadically), the substructure of the continents, the circulation of the mantle, and a cross section of the planet itself. Satellites equipped with extraordinarily accurate instruments adapted from optical and condensed-matter physics track not only the motions of the atmosphere but also the paths of the continents as they ride the surface of the Earth.

Biological Physics

The magnificent complexity of life studied by biology relies ultimately on the principles of physics. Physics, through its study of disordered systems, its application of statistics, and its sensitive instruments, is finally able to address such complexity from basic principles. Biology has so thoroughly integrated techniques like x-ray crystallography, electron microscopy, and most types of spectroscopy into its own research that their origin in physics is often forgotten.

Biological physics studies a range of scales from molecules to cells and membranes and on to cooperating multicellular systems like the brain.

Biologists can now use high-sensitivity instruments to study how individual molecules carry electric signals across the membrane of a nerve cell. The signals are electrical currents that flow by way of charged atoms, especially sodium, potassium, chlorine, and calcium. On the membrane of the nerve cell are certain protein molecules that can allow currents to get through the membrane. The protein molecules regulate current flow by folding into different configurations. Biologists want to know exactly how the channel opens and how it discriminates among atoms. Recently, physicists have developed instruments with enough sensitivity to allow them to measure the currents through individual channel molecules. These currents are only 1 part in 10^{12} of the current through an ordinary light bulb.

Principles of physics also underlie the entry of such foreign substances as hormones, antibodies, allergens, viruses, and bacteria into cells. On the surfaces of cells are proteins called receptors on which specific foreign substances can dock and enter. The structure of the receptor determines what substances can dock. Once a substance docks and binds to the receptor, the receptor moves the substance to certain sites on the cell membrane. At these sites the receptor carries the substance inside the cell. Now electro-optical instruments allow researchers to watch receptors bind substances and move along the membrane to the sites of entry.

One of biology's major occupations is the organization of the brain. The brain is a network of nerves, each nerve receiving input from many neighbors and sending signals to many others. To study neural networks, biophysicists adapt statistical-physics models that describe systems of many interacting atoms. Similar models describe the atmospheres of stars, the condensation of gases, and certain materials with random magnetic orientations called spin glasses. In the biological model, neural networks exhibit many of the same features that we find distinctive in our own brains: the networks occasionally lose information and forget it and are occasionally saturated with too much information and make mistakes.

Materials Science and Chemistry

Physics, chemistry, and materials science sometimes share content and techniques to such an extent that an outsider has difficulty distinguishing them. Here are some examples of their collaboration.

Surfaces of materials and the interfaces between materials. At the surfaces of solids and at the interfaces between solids and gases or between solids and liquids is where the physics and chemistry of oxidation, combustion, adhesion, precipitation, catalysis, and corrosion take place. Precipitation (the growth of a solid from a liquid) occurs at the interface between the solid and its solution. Catalysis, which speeds up chemical reactions, generates billions of dollars of revenue for industry annually. Corrosion destroys property causing annual losses in the billions of dollars. Researchers hope to understand these processes at the molecular level and to learn to control them.

Defects. No crystal is perfect. Somewhere in the orderly array of a crystal's atoms will be a breakdown in order. Such defects often determine the most interesting properties of a crystal. Certain defects produce the color in crystals. Still other defects indirectly strengthen crystals by inhibiting the sliding of crystal planes past each other. Many methods of fabricating semiconductors produce defects that reduce the semiconductors' effectiveness. Control of crystal defects is essential to the technologies of microelectronic devices, semiconductors, optical materials, catalysts, and all structural metals.

Amorphous and disordered materials. Glasses and plastics are materials whose atoms have little intrinsic order. These amorphous materials can be fabricated in ways that give them remarkable properties: they can transmit signals extremely quickly or have unique magnetic properties or possess remarkable strength. Glass fibers, for example, are increasingly crucial to our communications systems but can carry information at amazing rates only if they are smooth and free from impurities.

Organic electronic materials. Before 1972, no known organic compound conducted electricity. Now researchers have made several. Polyacetylene, for instance, is a compound of carbon and hydrogen; others are organic compounds combined with exotic metals. One organic electronic material is a polymer whose conductivity can be controlled over 12 orders of magnitude. Another is the first organic superconductor. Organic conductors might turn out to be lighter and cheaper than metals.

Mathematics

Mathematics has always been the language in which physics is expressed. In two particular areas, mathematics and physics are now even more deeply interwoven.

The movements of clouds, fluctuations in population growth, instabilities in a plasma that frustrate fusion, turbulence in fluid flows, motions of the atmosphere, and even the growth of a fern are all examples of the evolution of chaotic patterns. Mathematical representations of chaos have become vital to plasma, fluid, and condensed-matter physics and to physics' collaborations with geophysics, biological physics, chemistry, and materials science. Highly complex structures or motions can result from simply applying the same rules over and over on smaller and smaller scales. To help science describe these complexities, mathematical physicists are developing new mathematical approaches that can deal with chaos.

Topology is also increasingly important to elementary-particle physics and cosmology. The application of topology to particle-physics theory predicted two-dimensional defects in space-time called strings. The long-sought isolated north or south pole of a magnet, a monopole, can be viewed as a one-dimensional defect in the topology of space-time. Cosmologists consider that both defects might be crucial to understanding the structure of the universe.

For much of mathematical physics, the largest and fastest computers have become so essential that without them, further progress would be impossible.

Microelectronics

Transistors control the flow of electric current and do so reliably and cheaply. Within a decade of their invention, however, transistor circuits became too complex for conventional wiring techniques. Physicists and engineers found that they could make integrated circuits on a single slab of semiconductor and thus do away with individual components and most wires altogether.

Since 1962, the complexity of integrated-circuit chips has increased exponentially. Their sizes have decreased, and the amount of information that they can store has increased until a fingernail-sized chip today has the computing power that 20 years ago would have required a room-sized computer. Single chips for inexpensive computers now contain half a million transistors. Today, in this country, electronics and microelectronics comprise a more than a $100 billion per year industry.

Condensed-matter physics, chemistry, materials science, and microelectronics continue to be interwoven, each contributing to the advances of the other. New materials, like gallium arsenide, deposited in layers only a few atoms thick, promise circuits that can be faster than ever. New technologies provide the possibility of even smaller-scale, and therefore faster, microelectronics. New instruments able to look microscopically at the surfaces of materials, originally developed for research in condensed-matter physics, were quickly taken over by microelectronics manufacturers. Although microelectronics now is a scientifically mature technology, its future direction will continue to be profoundly affected by progress in physics.

Energy and the Environment

The connection between physics and energy has always run deep: combustion, fission, fusion, and solar energy are all based on the technologies and concepts of physics. Energy, in turn, has always had an impact on the environment; physics supplies the detectors that monitor that impact.

The technologies of fission (energy derived from splitting an atomic nucleus), fusion (energy derived from merging nuclei together), and solar energy (energy derived from the Sun) are all creations of basic research in physics. All are inherently capable of providing the world with virtually unlimited supplies of energy.

All three energy sources also depend on further advances in physics. Widespread use of fission as an energy source, because of escalating costs of the power plants, may be further in the future than once thought. Before fusion becomes an efficient energy source, physicists need to know more about the best means of heating the plasma, the growth of instabilities in the fusion plasma, and the effects of radiation on the fusion reactors. Solar power depends on a number of devices to convert sunlight into electricity; the devices in turn depend on materials from research in materials science, optics, and condensed-matter physics.

Meanwhile, combustion supplies 90 percent of the power generated in the world. Increasing costs and dwindling supplies of oil, gas, and coal have stimulated increased research in combustion and how it can be made more efficient. A 1-percent increase in the efficiency of automotive fuel consumption would save the United States around $1 billion per year.

Our ability to use gas, oil, and coal might ultimately depend less on its availability and more on its impact on the environment. Such problems as acid rain and the greenhouse effect could have serious consequences. The greenhouse effect results from burning fossil fuels, which has increased the concentration of carbon dioxide in the atmosphere. Over the next century, the carbon dioxide concentration in the atmosphere may double. Increasing carbon dioxide, in turn, influences the balance of incoming solar radiation and outgoing infrared radiation in such a way as to increase the surface temperature of the Earth, with an accompanying change in agro-climate zones that could affect world food production.

Optical Information Technologies

Optical technology might eventually have more impact on society than electronics. In electronic devices, electrons carry information along metal conductors. In optical technology, photons carry information along glass fibers. How much information electrons can carry depends partly on their range of frequencies: the higher the frequency, the more information can be carried. Photons can be modulated at much higher frequencies than electrons, and they can therefore carry millions of times more information; and glass fibers are lighter, cheaper, and are more immune to electrical interference than wires.

The process of sending information optically begins with a tiny rugged laser made of thin layers of a semiconductor. The laser sends light down a glass fiber the size of a human hair, which must be smooth and free from impurities and defects that would scatter or absorb light. The light signals weaken as they travel, and other semiconductor lasers the size of a salt grain periodically boost the signals. At the end of the fiber is a semiconductor that detects the light and converts it into electricity.

Shining light of certain frequencies at certain materials can change the way those materials absorb and transmit light. Physicists can use light to record patterns in such materials, thereby providing a means of storing and retrieving information optically. This technology allows the equivalent of several million encyclopedia volumes to be stored in material the size of a sugar cube. Another phenomenon, called optical bistability, permits physicists to switch one light beam on or off with another light beam. Optical bistability opens up the possibility of an optical version of a transistor, which in turn opens the door for optical computers.

The potential of optical technology seems limitless. Low-cost, high-speed information systems will be able to operate over long distances. Networks of powerful, inexpensive computers operating at near the speed of light promise to put each citizen in full, real-time color video contact with any other citizen, data base, business, or library. The new technology of computers with parallel architectures could combine with the new ability to make femtosecond pulses of light to make possible execution of nearly a quadrillion logical operations every second—orders of magnitude beyond today's best electronic computers.

National Security

Basic research and new technology arising from physics are essential to defense. The military advantage clearly lies with longer-range, better-sighted, more-intelligent defense systems. Physics has contributed to the development of long-distance radio communications, radar, rockets, infrared detectors, atomic clocks for precise navigation, smart weapons, nuclear arms, and acoustic, seismic, and optical techniques for detecting and destroying enemy submarines, artillery, and satellites. The ability to respond appropriately to a rapidly changing situation can depend on the ability to acquire and interpret vast amounts of data in real time. New optical and information-processing techniques are at the heart of this effort. The limitation and control of arms also depends on physics for verification techniques, and physicists have helped to draft verifiable arms-control treaties.

Medicine

Physicists and physicians have traditionally worked together. In fact, Röntgen made the first formal report of his discovery of x rays to a medical society. In the last decade, medical specialists have used the principles of physics to create techniques that help determine a body's internal structure without invading it.

The best known of the new techniques is computer-aided tomography, called CT or the CAT scan. CT gives physicians a view of the inside of the body in three dimensions. In CT, a sequence of narrow x-ray beams is aimed through the body. Each beam illuminates a line through the body. An image of a slice through the body can be reconstructed from data from these lines with the help of a computer. Images of several planes form a three-dimensional view.

Another imaging technique is positron-emission tomography, or PET. The image reconstruction principles of PET are similar to those of CT. In PET, physicians inject positron-emitting radioactive isotopes connected with a biological molecule that naturally concentrates in the part to be examined. From the gamma rays emitted by the positrons (positive electrons) the profile of that part of the body is reconstructed by a computer. PET makes possible generation of images of sites of specific biochemical activity, like the brain. Not only can PET detect abnormalities, like tumors, but it can also map the locations of different functions in the brain.

Magnetic resonance imaging (MRI) is another tomographic technique. Instead of x rays or positrons, MRI detects the body's protons, most of which are in water molecules. Protons spin on an axis like magnetic gyroscopes. When the body is put in a strong magnetic field, the axes of all the protons line up with the direction of the field. Then a radio-frequency field near the nuclear resonance frequency is added, making the axes of the protons wobble. Wobbling makes the protons reradiate the radio frequencies; tissues from different organs and from diseased organs all reradiate slightly differently. The radio signals are detected outside the body and reconstructed in a three-dimensional image by a computer. MRI allows construction of extraordinarily detailed images of soft tissues, which contain more water than hard tissues. In fact, bone is essentially transparent to MRI, allowing excellent pictures of the spinal cord. MRI is also used to make images of the working heart and of flowing blood.

Optical technology is increasingly essential in medicine. High-powered lasers can cauterize or even cut away tissue. Laser light will pass through the colorless cornea, lens, and interior of the eye to reattach a retina. Laser surgery is more precise and more sterile than surgery with a scalpel and causes much less bleeding and trauma.

3. MAINTAINING EXCELLENCE IN PHYSICS

Until World War II, physics was predominantly a European activity. But by the war's end the center of physics had moved to the United States. The United States has led the world in physics ever since, but within the last decade other nations have begun to resume their rightful positions. It is essential for the United States to stay at the forefront of knowledge and to maintain excellence in physics. Doing so is vital to our national interests because

- Physics provides the underpinnings of basic science that are essential for technology
- Physics generates techniques and concepts for other sciences
- Physics satisfies mankind's deeply felt need to understand nature at the most fundamental level.

In the United States, physics research is carried out in universities, in industry, and in federally supported laboratories. In some fields of physics, the research is organized around large facilities such as particle accelerators, plasma machines, synchrotron light sources, and satellites. However, in fields such as condensed-matter physics and atomic, molecular, and optical physics and interdisciplinary areas such as biophysics and atmospheric physics, most of the research is carried out by small groups, often working with equipment in their own laboratories. This variety of research styles and structures has yielded tremendous progress in physics. Continued cooperation among the various sectors carrying out physics research and a nurturing of the strengths of each sector is essential to maintain the high productivity of the nation's physics enterprise. Within each subfield, the peer-review system provides an effective mechanism to channel resources to the most productive researchers and the most promising endeavors.

Areas that bear watching in the coming years include revitalizing instrumentation in university research laboratories, maintaining adequate operating levels at existing facilities and building the forefront facilities of the next decades, strengthening small-group physics, and meeting the need for a new generation of physicists. Recommendations of the Physics Survey Committee addressing these and other concerns are summarized in the following paragraphs.

Recommendations

SMALL-GROUP RESEARCH

Research carried out by small groups plays a major role in the advance of science and in the education of scientists: it is responsible for over 70 percent of the physics doctorates that are awarded in this country.

Because the goals of small-group research are diverse, the contributions of individual research groups are often difficult to identify within the general march of science. If this research is viewed collectively, however, it can be seen to encompass some of the most challenging problems in physics. Furthermore, this research often lies closest to societal applications such as the creation of new materials or the development of new technologies for manufacturing or energy applications. Because of these facts, and because small-group research is the dominant mode for professional education in the universities, the United States must, in order to maintain excellence in physics, sustain a healthy research environment for small research groups.

- The Physics Survey Committee recommends that the general support for small-group physics be brought up to a level that reflects today's realities and that the federal program to provide up-to-date research equipment in university laboratories be broadened.

BASIC RESEARCH

Basic research is motivated by intellectual challenge. Nevertheless, over and over again, basic research has triggered technologies and applications that have galvanized our society. The impressive connection of basic research in physics with practical concerns is evident when one takes a broad view, as Section 2 demonstrates.

- The Physics Survey Committee recommends that the U.S. Department of Defense restore its investment in long-range basic research.
- Other federal mission-oriented agencies should support basic research in physics at levels appropriate to their missions.
- The federal government should encourage industries to support basic research, both in-house and in cooperation with universities and laboratories. Tax policy and antitrust policies should be modified to facilitate such collaborations.

MAJOR FACILITIES

Major facilities play a central role in important areas of physics. For physics to advance along the many lines of opportunity touched on briefly in this summary (and described in detail in the survey reports) the Physics Survey Committee recommends a number of new facilities, including:

- In elementary-particle physics, to penetrate farther into the fundamental nature of matter, construction of the highest-energy colliding-beam accelerator in the world, the Superconducting Super Collider;
- In nuclear physics, to study quark effects in nuclei, and to create and investigate a quark plasma, building of the Continuous Electron Beam Accelerator Facility and a relativistic nuclear collider;
- In condensed-matter physics, to study the structures, properties, and phase transitions in new materials at greater resolutions and under extreme conditions, construction of a new generation of synchrotron-radiation facilities and neutron-scattering facilities;
- In plasma physics, support of a vigorous research program to study the confinement and stability in fusion plasmas by both magnetic and inertial confinement methods; and to investigate the properties of ignited plasmas, the next major research frontier in magnetic fusion, building the Burning Core Experiment;
- In gravitational physics, to detect gravitational waves, construction of the Long-Baseline Gravitational-Wave Facility.

COMPUTERS

Although computers have not been discussed explicitly in this summary, their influence pervades physics. Computers are no longer simply extensions of desk calculators, tools for keeping and analyzing data. Large computers are now a means for testing and even creating theories in such areas as chemical kinetics, plasma simulations, galaxy dynamics, gravitational physics, and particle physics. In statistical physics and condensed-matter physics, the only possible solution to many basic problems is through computational methods. Computers have literally become extensions of the mind.

All fields of physics need greater computational capability: orders of magnitude more power, more interactive access between the computer and user, and more interaction between computer specialists and physicists.

MANPOWER

The supply of physicists with Ph.D. degrees and the demand by federal, industrial, and university institutions are, for the moment, in approximate balance. Ph.D. production has been stable for some years, but the number of U.S. graduate students has been slowly falling, the difference being made up by a steadily increasing component of foreign students. In the 1990s university physicists will start to retire at an increasing rate. To replace physics faculties with able young scientists, we must continue to attract students into physics and we must take steps to help universities attract new faculty. The following recommendations address these concerns:

- The number of predoctoral fellowships should be doubled to help reverse the decline in U.S.-born students.
- Government and industry, to help ensure future generations of physicists, should help attract bright young faculty to academia. Such programs as the Sloan Fellowships, the Presidential Young Investigator Awards, and the DOE Outstanding Junior Investigator Program should be expanded.
- Simplify the immigration laws for foreign-born physicists who wish to pursue permanent research careers in this country.
- Encourage more women and minorities to become physicists. Both are an invaluable resource, and both are dramatically underrepresented in physics.

INTERDISCIPLINARY RESEARCH

Physics has become increasingly essential to and dependent on advances in the other sciences and in industry and technology. To encourage discoveries at scientific interfaces, the following are recommended:

- Funding agencies should devise ways to evaluate and support interdisciplinary collaborations. Young faculty beginning interdisciplinary programs should be given special attention.
- Universities should promote interdepartmental, interdisciplinary research programs and centers. Such centers would also provide transdisciplinary education and attract interdisciplinary research funding.

FREE INTERNATIONAL EXCHANGE

International cooperation presents difficulties as well as rewards, and the outflow of technological information can be a cause for concern. But scientific secrets are not state secrets; they are held by nature. Our political adversaries are as free to try to learn them as we are, without violating national security. For science to flourish, scientists must be free to communicate and free to travel. Any interference with these basic principles is a loss for science and a loss for the offending nations.

APPENDIX 5—ADDITIONAL MATERIALS SUBMITTED FOR THE RECORD BY JON E. KALB

A "COVERT" PROCESS OR DUE PROCESS?

THE PEER REVIEW PROCESS OF THE U.S. NATIONAL SCIENCE FOUNDATION

by
Jon E. Kalb
Austin, Texas

Prepared for

Task Force on Science Policy
of the
Committee on Science and Technology
House of Representatives

A "COVERT" PROCESS OR DUE PROCESS? THE PEER REVIEW PROCESS OF THE U.S. NATIONAL SCIENCE FOUNDATION

by Jon E. Kalb, Austin, Texas

(Prepared for the Task Force on Science Policy)

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The peer review system is the cornerstone of NSF's funding process, that presently determines how over one billion research dollars are spent annually. The system is not perfect as nearly every researcher will agree to one degree or another. The question is, to what degree is the system imperfect and what can be done to improve it.

In many ways, however, the question eludes answering, at least for the researcher who is seeking grant funds. Reviewers of proposals are anonymous, and advisory panel meetings that evaluate proposals are closed meetings, so the influence of possible vested interests on the research proposal is unknown to the grant applicant. At that, no official minutes of a standard nature are in practice required of these meetings, and the "panel summary" may or may not reflect the actual ideas and recommendations of the panel. Instead, the NSF Program Director ultimately makes the final decision on proposals, subject to the approval of his or her superiors. Whether this "deliberative process" is in fact "due process" cannot be determined by the grant applicant, or by the public, since such information is protected by an exemption of the Freedom of Information Act (FOIA). Nor can internal records of declined proposals be examined through the Privacy Act, because of an exemption within this act that requires that such be part of a "system of records" for their disclosure, and because declined proposals are not part of a system of records. Thus, not only is due process not built into the peer review system in an open sense, the researcher has no way to determine whether due process was (or will be again) followed in the case of his or her grant application.

This is not to say that NSF does not have built into its system numerous checks and balances in the form of procedures, regulations, and codes that more often than not protect the researcher from abuses. What is apparent, however, is that even with these safeguards the researcher is vulnerable to what is probably the most common type of abuse, that of unfounded allegations of a damaging nature, or even slander in its many manifestations, by anonymous peers who in such cases more often than not have their own vested interests at stake.

Conflicts of interest are the bane of the peer review system, and it is this that most politicizes its operation, and characterizes its "covert" nature. In a legalistic sense such conflicts are "protected" by anonymity--by anonymous reviews, closed meetings, confidentiality codes, and a "closed" deliberative process, such that excessive or "abusive" competition is abetted by secretiveness that allows unsubstantiated charges to be given more credance than they would in an open system. It is the novice to the peer review system who is most affected by this, and least defended against established interests, the "elite" interests who are most informed as to the ways of the decision-making process.

For all these reasons I would not presume that the history of the case I have described in the accompanying report* to be atypical of other funding programs at NSF. I feel that such abuses are inherent in the system and that by definition they are pervasive, to one degree or another. In general, the matter of degree is something that cannot be accurately measured because the potential loss to science of a declined proposal that is more worthy than another, of research not conducted, is "immeasurable." The potential loss to science of one out of ten proposals--or whatever the actual figures are--that are more worthy is not only a loss to science, it is potentially a major waste of research dollars, moreover those that are being used to finance scientific output that is not fully won through "open" and equal opportunity.

Recommendations

From my experience, it is apparent that NSF is at a loss to determine how to "right" abuses. In a sense this too is built into the system, because if NSF acts against a scientist for reasons of conduct or ethics, by say, cutting off his or her research funds, then it is making a unilateral decision which is contrary to the pluralism principle of the peer review process.

*A History of U.S. National Science Foundation-Sponsored Prehistory Research in Ethiopia, 1973-1983--Questions and Issues.

1. My central suggestion for improving the peer review system is to make a step by step re-examination of the legality of the process, particularly regarding due process and whether a means or a procedure can be established to "guarantee" or demonstrate to a researcher that such has or will be followed in the course of his or her grant application. This means to examine both the "internal" and the "external" path of a proposal, hand-by-hand, from its arrival at NSF's Central Processing Center, through its circulation to outside reviewers and panelists, to its ultimate destruction at the Federal Records Center. As examples, to examine how the record keeping of a proposal complies with the Federal Records Act or the Federal Reports Act, whether the conflicts-of-interests guidelines for reviewers and panelists are really adequate as presently described in the Code of Federal Regulations, whether the panel procedures are fully in accordance with the Federal Advisory Committee Act, whether the procedures for release of information about proposals, and researchers, can be improved upon through the case law constructions of the FOIA and the Privacy Act, and in general whether the peer review process, as a "covert review process," fundamentally complies with the basic due process tenant of the Fifth Amendment of the Constitution.

2. I feel that such a legal examination should pay particular emphasis to examining ways of notifying researchers about what "facts" peer review committees are reviewing, to

insure that researchers have some opportunity to rebut factual allegations that are being considered in the course of a decision on his or her grant application. In other words, such fundamental due process safeguards should be built into the peer review system as a matter of course.

3. I would particularly suggest that this legal examination be conducted by specialists outside of NSF and that it include both legal experts as well as practicing scientists. One outcome of such a study would hopefully be ways to accelerate, and clearly advertise, the system of appeals at NSF, especially regarding conflicts that arise in the course of the evaluation of proposals, as well as in funded projects that directly relate to NSF procedures, as spelled out for example, in Chapter VI ("The National Science Foundation"), Title 45 of the Code of Federal Regulations. In doing so, safeguards should be established, perhaps modeled after civil codes, to protect the appellant from unwarranted or vindictive measures of a retaliatory nature from those on the opposite end of appeals. Perhaps a more objective means of administrating appeals would be to augment and broaden NSF's Office of Audit and Oversight, particularly through rotating officers in charge of such matters. Appellants should also be given a defined recourse for final appeals with oversight offices or committees outside of NSF.

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Though increased "legalization" of the peer review system, "reform" for a better term, is a means to improve the process, it will never remove the system from abuses. Abuses will always exist and it is on this level that the intervention of the professional scientific and academic community is required. It is ultimately the tolerance or intolerance of this group that either fosters or hinders the "character" of science and its institutions and makes it and us what we are. Whether it is a Code of Conduct or a Code of Ethics that is needed, or whether it is simply an increased level of awareness of what-is-needed is a decision that must be made collectively and individually.

There are few who would deny that scientific research of an institutional nature is more often than not a political animal of the first order. Science is intensely political--there is competition for funds, for students, for laboratories, for publications, for tenure, and for scientific discovery. However, because much of what scientists do is conducted behind closed doors, the rules that they go by are not always the same rules, or "legal" codes say, that govern the actions of the businessman or a legislator. Most of what scientists do is based on the judgment of peers, through peer review systems of one form or another. The "rights" of such systems are clear enough, but the "wrongs" are harder to come by, and make the scientist and the educator alike vulnerable to abuses, which foster discontent and frustrations, and decrease performance levels and the quality of output. If we are to prevent such abuses, therefore, we have to confront wrongs at all levels, and actively and openly work to challenge and improve the system.

A CASE STUDY OF THE PEER REVIEW PROCESS OF THE U.S. NATIONAL SCIENCE FOUNDATION-- PREHISTORY RESEARCH IN ETHIOPIA

by Jon E. Kalb
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Prepared for the
Subcommittee for Investigations and Oversight
Committee for Science and Technology
U.S. House of Representatives

1986

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In late 1976 three associates of mine submitted two grant applications to the U.S. National Science Foundation (NSF). The proposals, submitted from Southern Methodist University (SMU) and New York University (NYU), were interlinking in purpose and requested funds to conduct prehistory studies in Ethiopia under the umbrella of an Addis Ababa-based research organization that I directed (while also living in Ethiopia, as I had been since 1971). The organization operated under a permit from the Ethiopian government that allowed it to conduct investigations on some of the most important prehistoric sites in Africa, sites that my American and Ethiopian colleagues and I had discovered over the previous two years.

The SMU and NYU proposals requested funds from NSF's Anthropology Program, within the Division of Behavioral and Neural Sciences, and were to be evaluated through the peer review system. This system requires that proposals be judged on their merits by "outside reviewers"--by knowledgeable experts outside of NSF--and by an advisory panel composed of experts from around the U.S. The panel meets periodically at NSF to evaluate the outside reviews, as well as those of one or two "internal" reviews by panel members who are assigned particular proposals. The panelists then give their recommendations to the chairperson of the panel, who in this case was the Director of the Anthropology Program. It is the Program Director's responsibility to decide whether proposals are to be funded, based on the funds available in the program and other considerations. Final approvals must then be given by more senior NSF officers.

One of the outside reviewers of the SMU and NYU proposals reported to the Program Director allegations that I was associated with the U.S. Central Intelligence Agency (CIA), which I am not nor ever have been in any way whatsoever. [The allegations had previously been raised with the Anthropology Program by a "rival" whom I had been critical of over ethical matters in a well known dispute that NSF was involved with in 1974.] During the panel evaluation of the proposals, in early 1977, the Program Director introduced the question of the CIA allegations, which were central to all following discussion of the SMU proposal, and by association, of the NYU proposal. The scientific merits of the SMU and NYU proposals were discussed only secondarily to the CIA question, even though the outside and internal panel reviews of both proposals were unanimously excellent. The Program Director felt that NSF's name and the profession of anthropology could not be blackened by a researcher with CIA associations. The panel meeting ended when the Director "tabled" the proposals and told the panelists that she would investigate the matter further, with the State Department or elsewhere, as to my possible associations. No vote or consensus was taken on the merits of the proposals, and as it turned out the proposals were not introduced to the panel again. Prior to the panel meeting, I was never notified about the CIA allegations, nor was I given an opportunity to refute them.

However, over a six week period following the panel meeting, the Program Director made wide-ranging inquiries about my possible associations with the CIA within the government and the academic community. According to her own account, she did so with the approval of her superiors, and with NSF's General Counsel Office. As might be expected, the Director could come up with no concrete "evidence" that I was not connected with the CIA. In the course of her inquiries, however, questions were raised about a possible covert source of funds that had financed my "activities" in Ethiopia, my "connections" in Addis Ababa, and whether I had "infiltrated" the Ethiopian government.

Following the Program Director's "investigation," the SMU and NYU proposals were declined by the Anthropology Program. The "official" reasons cited in the "'Panel' Summary" made no mention of the CIA issue. Rather, the principal cause given for the rejection of the SMU proposal was that the principal investigator, a professor of anthropology from SMU, was involved in another project, in Egypt. The professor's involvement in a second project, though not uncommon for senior scientists, raised a legitimate question; however, it was not one to which the panel gave major concern. "Unofficially" the Program Director went out of her way to tell the professor that there were two reasons for the decline of his proposal: first, his involvement in the Egyptian project, and second, the allegations of my involvement with the CIA. The reasons later given for the rejection of the NYU proposal were that it was tied to the SMU proposal, and that the political situation in Ethiopia had--some weeks after the panel meeting--significantly worsened.

After the SMU professor was told of the decline of his proposal he complained in writing to the Program Director about the manner in which the CIA question had been handled--that it had overtones of "McCarthyism." The professor requested that the issue be taken to the Director of NSF. Apparently nothing was done.

After I was told of the decline of the proposal, and of the "unofficial" reasons for it, I requested from the Director of NSF that the proposals be reconsidered. In early 1978 this resulted in a meeting I had with an Assistant Director of NSF. The Assistant Director stated that there was "no evidence in the files" that the CIA allegation had entered into NSF's decision to decline the proposals.

In mid-1978, I requested from NSF under the Freedom of Information Act (FOIA) copies of any records pertaining to the subject matter of my meeting with the Assistant Director of NSF and to my communications with NSF concerning the CIA issue and my request to have the proposals reconsidered. NSF replied that no such records could "be identified." As I later learned (in 1983), however, no effort was made by NSF to search the pertinent files in the case, the proposal files.

Shortly thereafter I was expelled from Ethiopia by the Ethiopian Department of Public Security and my research organization collapsed. No reason for my expulsion was given; however, the rival who had initially taken the CIA charges to NSF had also promoted these same charges with Ethiopian officials.

From late 1977 to late 1981, NSF's Anthropology Program embarked on a diplomatic and "fund-raising" campaign--approved at the highest levels of NSF--to help secure a "concession" from the Ethiopian government for the prehistoric sites previously studied by my organization, on behalf of "American interests," led by the same interests that had raised or promoted the CIA allegations with NSF. NSF's investment in this matter went so far as to give three consecutive non-peer review grant "awards" to a senior scientist for this purpose. In late 1982, however, the Ethiopian government suspended indefinitely all prehistory research in the country pending an examination of its policies concerning such research, and following complaints relating to this matter by former Ethiopian employees of my organization (who had also been implicated over the CIA charges).

In mid-1982, I wrote the Vice President of the U.S. summarizing my case with NSF (as I then understood it), and requestioning his assistance in securing from NSF a public apology for giving the CIA allegations credance (without due process) and for contributing to their dissemination during the peer review of the 1977 proposals. An inquiry from the Vice President's Office to the Director of NSF resulted in an internal investigation by NSF, in part by the same NSF officers who had been involved with the affair originally. A report from the Director of NSF's Office of Audit and Oversight to the Office of the Vice President said there was "no evidence" that the CIA question played any significant role in the decline of the proposals.

Documentation

From late 1982 to early 1985 I made 115 separate FOIA and related requests to NSF bearing on this affair and on all aspects of NSF's funding for prehistory research in Ethiopia from 1973-1983. I also interviewed the panelists that "evaluated" the SMU and NYU proposals as well as various former NSF officials. I have reconstructed and documented the case in detail, that includes some of the (duplicated) "lost records" that explicitly deal with the CIA question. I have also unraveled what I feel is a pattern of permissiveness and malfeasance surrounding NSF's funding in general in Ethiopia. This information is contained in a 137 page report entitled, "A History of U.S. National Science Foundation-Sponsored Prehistory Research in Ethiopia, 1973-1983--Questions and Issues," that contains 1200 references and footnotes documenting all aspects of the case and of NSF's policies concerning Ethiopia.

Finally, I have outlined what I believe are inherent problems in NSF's peer review system, and have presented this and some recommendations for improvement of this system to the Science Policy Task Force with the U.S. House Committee for Science and Technology. These ideas are contained in the report entitled, "A 'Covert' Process or Due Process? The Peer Review Process of the U.S. National Science Foundation."

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